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ADVERTISEMENT.

The Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions;* which was accordingly done upon the 26th of March, 1752. And the grounds of their choice are, and will continue to

be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they receive them, are to be considered in no other light than as a matter of civility, in return for the respect shewn to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities, of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the highest applause and approbation. And therefore it is hoped, that no regard will hereafter be paid to such reports, and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

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The PRESIDENT and COUNCIL of the ROYAL SOCIETY adjudged the Gold and Silver Medals on COUNT RUMFORD's Donation, to SIR HUMPHRY DAVY, LL. D. F. R. S. for his Papers on Combustion and Flame, published in the last Volume of the Philosophical Transactions.

PHILOSOPHICAL

TRANSACTIONS.

I. *An Account of the circulation of the blood in the class Vermes of Linnæus, and the principle explained in which it differs from that in the higher classes. By Sir* Everard Home, *Bart. V. P. R. S.*

Read November 7, 1816.

HAVING spent a part of last August on the Sussex coast, where the Lumbricus marinus is met with in great numbers, it suggested itself as an object worthy of investigation, to determine the difference in structure between this worm and the Teredo on the one hand, and the Lumbricus terrestris on the other. I was the more led to this from having made myself familiar with the structure of the Teredo, an account of which has a place in the Philosophical Transactions.

These three different genera of worms, although they differ in many respects from each other, have several points of resemblance; they all destroy by boring the substances they inhabit, an action requiring great muscular power; they take into their stomachs the broken down substance; they have gizzards, and red blood; their place of residence however, being very different, requires that there should be peculiarities in

each of them, one of these is, the mode in which the blood is aerated.

We are not in my opinion furnished with a sufficient stock of materials in comparative anatomy, to make out a correct arrangement of the whole system of nature, nor do I know the best plan upon which it can be made; but, at present, I look upon the circulation of the blood, and the mode of aerating it, as one liable to the fewest objections.

The brain and nerves, as they are the most essential organs in the animal œconomy, appear to have a prior claim, but the difference of structure in those organs, and in the spinal marrow, is too small to serve for this purpose.

The heart and blood vessels are the parts next in importance, and necessarily vary more in their structure, so as readily to give characters to a greater number of classes, which is a great advantage. I have made these remarks from a desire that the science to which I have devoted much of my attention, should be pursued by those who engage in it, in the manner most likely to bring it to perfection, which is by submitting to the drudgery of making out the structures of animals not yet known, instead of grasping at the whole system, so many parts of which we are unacquainted with. This attempt resembles that of the giants of old, in the allegory, who foolishly believed, when they got hold of the lower links of the golden chain which hung down from heaven, that they had acquired the means of getting possession of the power by which it was suspended.

The circulation of the blood in the Lumbricus marinus, is probably the same as in all the vermes with external organs of aeration. The transparency of the animal shows the action

of many of the blood vessels, and the course of the blood, but in some parts they are hid from our view, and are only to be detected by sudden coagulation of that fluid, which is effected by immersion in vinegar. I most readily confess, that had not Mr. Clift, the Conservator of the Museum of the Royal College of Surgeons in London, made sketches of the parts while in action, and given me his assistance, I probably should have failed in the investigation. The blood is brought from every part of the body to a common trunk that supplies the organs of aeration, which are 26 in number, but does not all pass through them, a portion of unaerated blood going on towards the tail.

The blood is propelled from the blood vessels of the organs of aeration with great force, these vessels performing the function of the ventricle of the heart in other animals; it is carried to a large artery on the back in an aerated state, passes towards the head, from thence it is returned by a corresponding vein on the belly, and before it arrives again at the organs of aeration, this vein receives supplies from two auricles furnished by the veins of the viscera; but there is no ventricle between the auricles and these organs.

In the Lumbricus terrestris there is no heart, and the organs of aeration are not external, but consist of small lateral cells with an external opening, as in the leech, so that they can take no part in propelling the blood; that office is entirely performed by the muscular power of the coats of the arteries. In this animal the circulation is very simple; the artery upon the back, by its action, forces the blood up to the head, and it is returned by a corresponding vein upon the belly; near the head there are five pair of lateral canals of commu-

nication between the artery and vein, which being kept full, furnish a supply of blood to be used when necessary, and admit of a greater or less proportion going to the head, or being returned by them to the vein, as occasion may require, their coats being exceedingly elastic.

From these observations on the circulation in the Lumbricus marinus, and Lumbricus terrestris, and those formerly made on that of the Teredo navalis, these genera appear to form three links in the chain of gradation of animals, and have led me to the belief, that the striking difference between the circulation of the blood in all the vermes, and that of the higher classes of animals, may be explained, and shown to answer an essential purpose in their œconomy.

In explaining my opinion, I shall make myself better understood, by reviewing in a summary manner the modes by which the circulation is carried on in the different classes of animals; this will also enable me to show that a classification of animals will at least be as perfect by taking the circulation of the blood for our guide, as the brain and spinal marrow.

In all animals of the class Mammalia, there is a complete double circulation; in the one, the blood is aerated; in the other, the body is supplied; they correspond in velocity, the aeration is great, the heat of the animal is kept up to a certain degree, and, if the action of the heart both in the auricles and ventricles has once entirely ceased, it cannot be restored. In birds, the circulation is completely double, but the aeration of the blood is less than in the mammalia, the lungs being smaller, and their cells larger; when the action of the heart has entirely ceased, it cannot be restored.

In the Amphibia the circulation is double, in appearance, but partially so in reality, the septum between the ventricles having apertures communicating from the one to the other; this structure renders the mass of blood less aerated, and the circulation less dependent upon the organs of aeration. Under these circumstances, the blood varies in its temperature with the atmosphere, and life is carried on under very imperfect degrees of aeration of the blood; but, when the action of the heart has entirely ceased, although the individual muscles of which it is composed can be irritated to produce contractions, for days, and weeks, yet the complete action of the whole organ cannot be restored.

In fishes the circulation of the blood is not double, as in the higher classes. The heart is composed of an auricle and a ventricle; the one is employed to receive the blood that has been used for the support of the body, and the other for propelling it through the organs of aeration; the aerated blood is collected into one artery, and passes to the different parts of the body for their nourishment, with no other impulse than what can be produced by the muscularity of the coats of the arterial system. It is true that the red blood does not go far towards the extreme parts. Even in this class, the circulation cannot be restored after the heart has been entirely at rest. S ories are told respecting fishes sold at market in North America in a frozen state, which, after they were carried home and gradually thawed, have been seen to move: to this I can very readily subscribe, since I have seen parts of quadrupeds completely frozen so as to be made solid, then thaw and recover. I have seen blood converted into ice in its blood vessel, thaw and become fluid, yet afterwards coagulate. I have also seen a carp after the heart and viscera were removed,

at the distance of many hours, when exposed to heat, leap to a considerable distance; but, when once the circulation has ceased, there is no authentic account of the circulation being restored. The lampreys have a less degree of aeration of the blood than fishes, and in that respect become an intermediate link between them and vermes; they have less the habits of muscular exertion, which may explain their having a less degree of aeration of the blood.

The vermes of LINNÆUS is a class made up of materials, which, in the present view of the subject, must be divided into five distinct orders. Those animals in which there is a heart; those in which there is no heart, but external organs of aeration; those in which the circulation is carried on by the arteries and veins of the body, there being neither heart nor external organs of aeration; those in which the blood does not circulate, but in which an undulation is kept up, a circulation for the purpose of aerating the blood being rendered unnecessary, as the aerating organs consist of air tubes that ramify through every part of the body, and those in which neither circulation nor undulation can be demonstrated.

In all the classes of animals above the Vermes, the heart is employed to receive unaerated blood, and to propel that blood into the organs of aeration; and in fishes, this is the only office it performs; but, in the class Vermes, the circulation is completely reversed, as I have formerly explained in the teredines, since the aerated blood goes to the heart, which propels it to the different parts of the body.

In so small an animal as the Teredo navalis, all the peculiarities of this kind of circulation were not readily made out, but by examining it in the sepia officinalis of a large size, I find that there is the same change in the office of the blood vessels

of the organs of aeration, as of the heart itself, since the vessels that carry the blood to these organs are larger and weaker than those which return it to the heart, so that, instead of the blood being propelled into the organs of aeration by arteries, it is carried by veins, and propelled towards the heart by arteries.

In proof of the correctness of my description of this kind of circulation, which I consider to be common to all that order, I have annexed two drawings of the heart of the sepia officinalis; they were made in the year 1787, by Mr. Bell, draughtsman to Mr. Hunter, and the preparations from which they were taken are preserved in the Museum, and it is with the permission of the Board of Curators that the present copies are laid before the Society.

In the sepia, whose veins are of an enormous size, there is a bulb or swelling of the vein at the root of each of the organs of aeration, with a double valve to prevent regurgitation of the blood after it had once entered these organs; to this is connected a spongy body, that does not appear to communicate with the bulb itself, the use of which I am unacquainted with. This peculiarity appears to be wanting in the teredines and other vermes, as not being required.

The circulation of the blood in the lowest class of animals being the reverse in principle, to what it is in the higher classes, led me to consider, from what circumstances this change could be produced: and reflecting that the great difference, between animals of the higher classes and those of the vermes, is, that when the heart stops in the one, the animal dies, but in the other, that this action can be restored, it led me to believe, that the peculiarities in their circulation produce the means by which the action of the heart is renewed. Whether all the vermes upon any occasion go into a torpid

state, in which the circulation ceases, or what portion of them is in the habit of doing so, is no part of the present consideration; that some of them do, is sufficiently well ascertained, and the mode of their reanimation appears to arise from the air confined in the organs of aeration escaping, and fresh air being received, the effect of which, probably, is to excite the arteries of those organs to action, and consequently to send a supply of aerated blood to the heart. When the garden snail is shut up in the winter, all external communication is excluded, and therefore for months the action of the heart and of the organs of aeration must have ceased; when warmth and moisture are applied, the membranous films fall off, a globule of air that had been inclosed in the organs of aeration becomes rarified, it expands and forces its way out, and thus admits fresh air to be applied to the arteries of these organs.

In the second order of vermes, the external organs of aeration must cease to act, whenever the body of the animal is inclosed in sand, and will have their action restored as soon as fresh sea water is applied to them.

In the third order, in which the circulation consists only of arteries and veins, they also probably cease to act whenever the organs of aeration are not supplied, and renew their action whenever fresh supplies are received.

In the fourth order, the action of the blood vessels is scarcely necessary for the functions of life, the air vessels carrying the air to the blood, and retaining a considerable supply.

In the fifth order, the aeration of every part of the substance of the animal appears to be a substitute for any particular fluid having a regular circulation.

EXPLANATION OF THE PLATES.

Plate I.

Exhibits a posterior view of the heart, aerating organs, and great veins in the Sepia officinalis.

AA. The vena cava anterior.

BBB. The venæ cavæ inferiores.

C. The vena cava media.

DDDD. Large canals officiating as veins, by receiving the blood from the different venæ cavæ; they have appendages of a peculiar kind like small grapes, which are hollow, and communicate by large orifices with their cavity. These are additional reservoirs, but they also secrete something that has a yellow tint.

EE. Two large venal trunks going to the organs of aeration.

FF. Two bulbs, in each of which is a pair of valves to prevent regurgitation of the blood from the aerating organs.

GG. Two small hollow spongy bodies which appear to have no direct communication with the bulbs to which they are attached.

H. The great vessel, which from its size I call a vein, going to supply the aerating organs.

I. The corresponding vessel, which from its size I call an artery, by which the blood goes from the aerating organs to the heart.

KK. The two auricles of the heart.

L. The ventricle.

M. The aorta.

NN. The inferior aorta.

OO. The aerating organs.

PLATE II.

The same parts shown in an opposite point of view.

AA. The vena cava anterior.

BBB. The venæ cavæ inferiores.

DDDD. The large veins with their appendages described in Pl. I.

EE. The ligamentous attachments, hiding the origin of the large vein that goes to the organs of aeration.

FF. The bulbs belonging to the veins that go to the aerating organs.

GG. The spongy bodies attached to the bulbs.

II. The arteries that return the blood to the heart.

KK. The two auricles of the heart.

L. The ventricle.

NN. The inferior aorta.

OO. The aerating organs.

PLATE III.

This plate exhibits three figures of the Lumbricus marinus, and one of the terrestris.

Fig. 1. The Lumbricus marinus as it appears when in full vigour, playing about in salt water. The mouth has the lips turned out, as if in search of food.

Fig. 2. The body of the animal is laid open, giving a view of the blood vessels which lie upon the back under the skin, and supported by the stomach and intestine on which they rest.

aaa. The great artery running from the tail to the head.

bbb. The nerve which lies upon it.

cccc. The external organs of aeration.

dddd. The blood vessels which I call arteries, that bring the blood from the aerating organs to the artery; under these are seen the veins which convey the blood to these organs.

eeee. Five very vascular bodies, probably answering the purpose of a liver.

f. The œsophagus.

g. The stomach.

hh. Two bags that communicate by small apertures with the stomach.

ii. The intestine laid bare, but not opened into.

kk. The two auricles lying upon the intestine just where it begins to swell out, one on each side.

ll. The two lateral veins that supply the auricles.

mm. Ova distinctly ascertained to be such.

Fig. 3. The skin longitudinally divided on the belly of the animal, and turned back so as to expose the parts immediately under it.

aaa. The vein corresponding to the artery on the opposite side.

bb. The termination of the two auricles in this vein, by infundibular vessels.

cccc. The vessels or venal branches going to the organs of aeration.

eeee. f, g, hh, ii, mm, the same as in the last figure.

Fig. 4. A representation of the arteries in the Lumbricus terrestris.

The animal is laid open by a longitudinal incision through

the skin of the back, which is turned aside, exposing the stomach and intestine upon which the artery lies.

aaa. The artery in which the blood has its course towards the head.

bbb. The five lateral canals, by which it communicates with the vein on the belly. These are inclosed as it were in separate cells.

cc. Œsophagus.

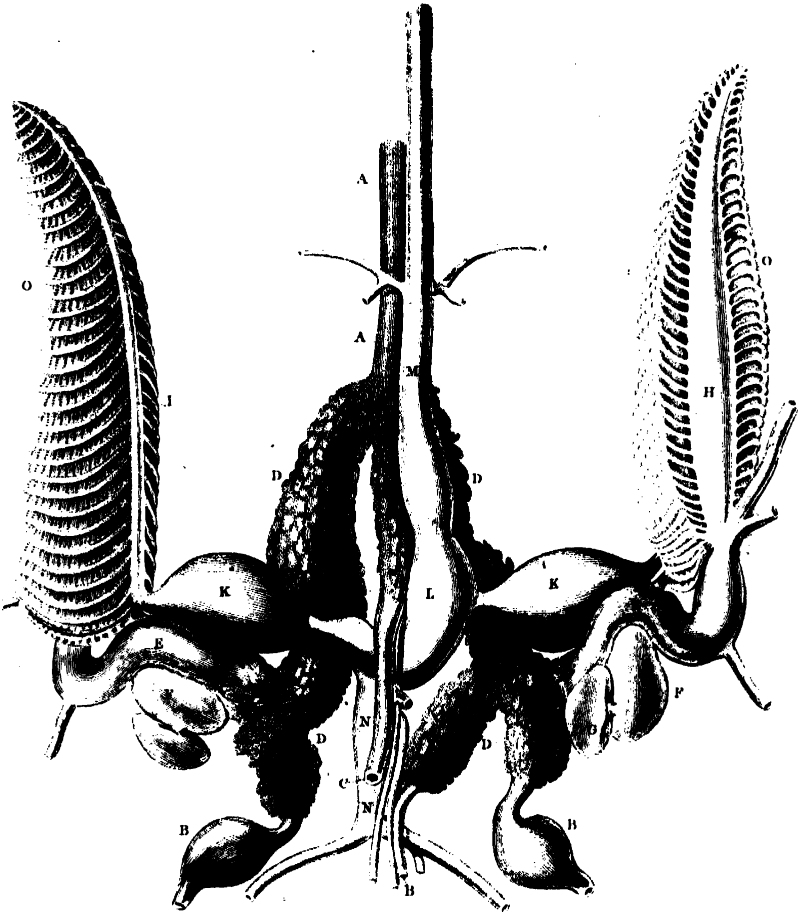
d. Crop.

e. Gizzard.

fff. The intestine made to put on a loculated appearance by transverse bands, which fix it in its situation.

gg. Ova.

hhhh. The organs of aeration, consisting of cells, with openings through the external skin.



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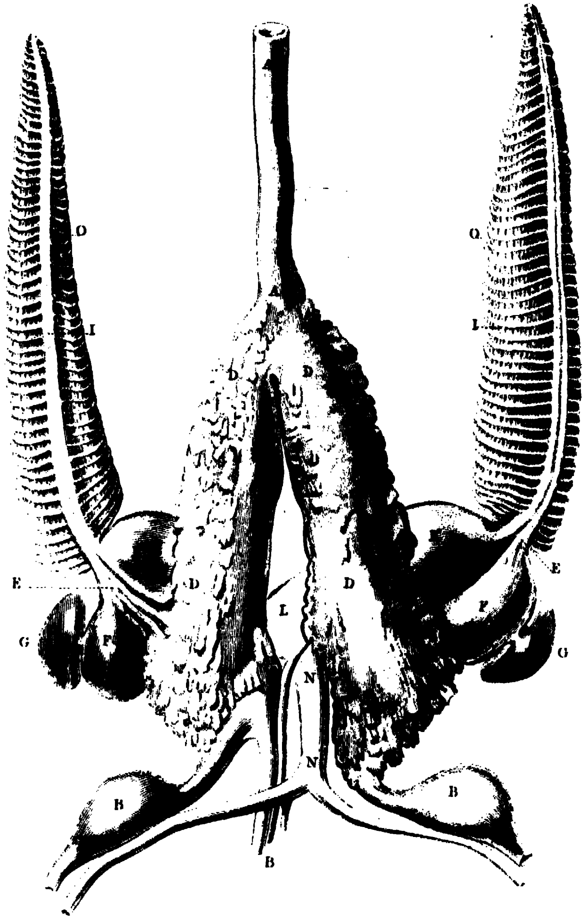

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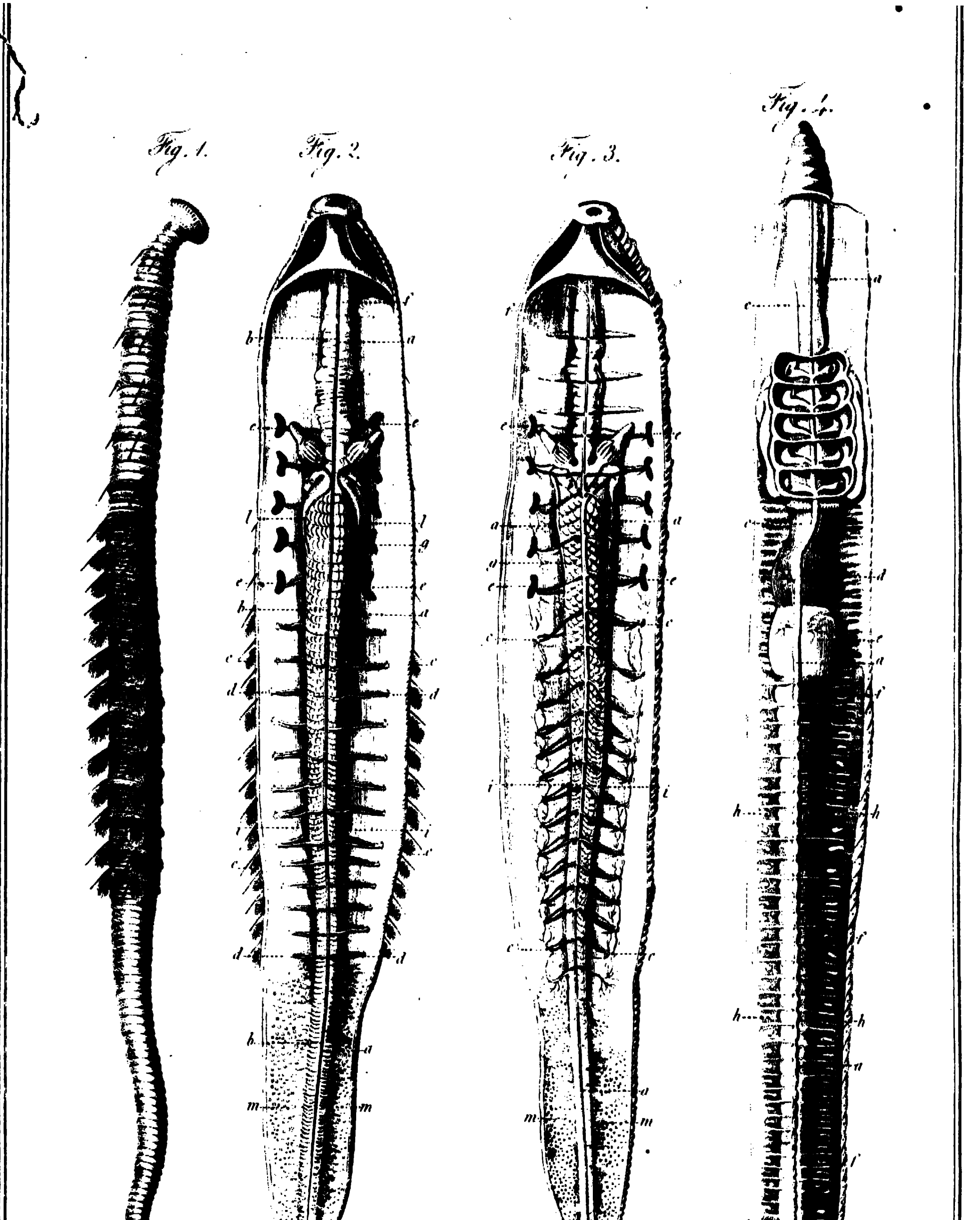

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II. *Observations on the Hirudo vulgaris. By* James Rawlins Johnson, *M. D. F. L. S. &c. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G. C. B. P. R. S.*

Read Nov. 14, 1816.

STRONGLY impressed with the conviction that every attempt to elucidate any part of natural history, will meet with a favorable reception, I have ventured to submit to the notice of the Royal Society, a few observations relative to the mode of propagation, &c. of the *Hirudo vulgaris*.

This little animal (Pl. IV. Fig. 1, 2.) is found in great abundance in rivulets, attached to the under surface of stones, and in those situations where it is little exposed to the action of the current. It varies as to its length, from an inch to an inch and a half, is of a dark brown colour on the back, marked with numerous transverse lines, but of nearly one uniform colour on the belly, chiefly that of a yellowish green. A central line of a black colour passes both on the back and belly, from the head to the tail. The ground colour, however, differs, hence several varieties have been enumerated.

From the circumstance of its having eight eyes, which are delineated, magnified, in Fig. 4, it has been denominated by LINNÆUS and others, *Hirudo octoculata;* but the *Hirudo tessulata* presenting an equal number of eyes, we shall give the preference to the name under which we find it described in MULLER, that of *Hirudo vulgaris*.

It appears, from the statement of an eminent naturalist, lately deceased, that this animal possesses the power of

reproduction in almost an equal degree with the polype: but the experiments I have hitherto made, by no means establish this point.

In its structure the *H. vulgaris* closely resembles the *H. medicinalis.* At the extremity of the tail we find the *anus.* There are four vessels destined to convey the circulating fluid; a dorsal, an abdominal, and lateral vessels. These tubes carry red blood, and have a well marked systole and diastole. Eight pulsations occur in the course of a minute. I have frequently placed the *H. vulgaris* under the microscope, in order to discover a central organ of the vascular system, or what corresponds to the heart, but without effect. According to the opinion of some physiologists, the several dilatations occurring in the course of the abdominal blood vessel, which, in the present instance, assume the figure of a diamond, (a portion of which is seen, magnified, in Fig. 5.) answer very effectually this purpose.

The food of the *H. vulgaris* consists of the smaller kind of earth-worms, &c. which, in like manner with the *H. sanguisuga*, it swallows whole.

In the summer of 1815, I kept several of these animals, in order to discover their mode of propagation. Examining the vessels that contained them, from time to time, I found them to be oviparous: the ova being enveloped in a gelatinous mass, surrounded by a firm membrane, to which we attach the term of capsule. These ova I preserved many months, but they proved unproductive. Disappointed in this my first attempt, I resolved to continue my research, and again collected during the last summer, a considerable number; when the object I had in view, was fully attained.

About the middle of June, several capsules were deposited. As some of them were transparent, I had an opportunity of examining their contents. I could distinctly observe the ova in them; then had the satisfaction of identifying animal existence; and ultimately, of tracing the young from this period to their exclusion.

Having thus found the ova productive, I examined the vessel daily, with a view of marking the time when they were deposited, and the period required to produce the changes I have noticed.

On the 4th of August, I observed a capsule, in which the ova were very distinct: on the 26th of the same month, animal existence was developed, and on the 17th of September the young were excluded. In this instance, signs of vitality were manifest in twenty-two days, and in forty-four days, the young had escaped.

On the 14th of August, another capsule was deposited, in which animal existence was evident on the 1st of September, and on the 24th of the same month, the young were excluded. In this case, the first sign of life was traced in eighteen days, and the young had escaped in forty-two days.

August 13th. I observed two of the *H. vulgaris* in *actu coitus*, and found them to copulate after the same manner as the common snail. In this state I removed and kept them apart from the rest. On the following morning they had separated, when I consigned them to different vessels. One of them, shortly afterwards, escaped from its confinement, and was lost. It, however, produced two capsules, one on the 17th of August, the fourth day after copulation, and the other on the 18th; both which proved unproductive. The

other leech produced a capsule on the 17th of August, the fourth day also after copulation, and one on each of the following days: 18th, 21st, 23d, 24th, 27th, 30th, September 4th and 8th: on the 2d of October, it died. In the whole it deposited nine capsules: of these, two only were productive. The one, (17th August) indicated animal existence in twenty days (6th September), and in fifty-six days (12th October), the young were excluded. In the other (August 18th), animal existence was developed in twenty-one days, (September 8th), and in sixty days (17th October), the young made their escape. Thus, traces of vitality were manifest in each of the capsules about the same time as in those previously mentioned; that is, in three weeks: but, from this period to the exclusion of the young, five weeks had in the latter instance elapsed, whereas, in the former, this occurrence took place in the space of three weeks. This I can only account for, by stating, that the ova deposited on the 4th and 14th of August were exposed to the sun, whilst those produced on the 17th and 18th of the same month, were kept constantly in the shade.

We shall now describe the manner in which these capsules are deposited.

When the *H. vulgaris* is about to produce one of these bodies, it is observed to be greatly contracted both above and below the uterus. After having fixed its tail, which it does not once remove during the operation, it in the course of ten minutes presents the appearance represented in Fig. 3. At first, there is no difference as to colour between the distended portion and the rest of the body; but, in a few minutes, this part becomes of a milky-white colour, from the formation

of a film or membrane, into which the animal forces, with some effort, the whole contents of the uterus: This done, the *H. vulgaris* elongates the anterior portion of the body, and thus loosening the enveloping membrane, withdraws its head from it, as from a collar. In some instances, where this membrane cannot be readily detached, I have observed the animal to bend back its head, and then taking it in its mouth, and drawing it gently, is thus enabled to remove it. From the first formation of this membrane or capsule, to its removal from the body, twenty minutes usually elapse. It is, at this time, very elastic, and of no determinate figure. After the *H. vulgaris* has firmly fixed it to some surrounding substance, it fashions it with its mouth, until it presents an oval form, such as is delineated in Fig. 7. It afterwards returns once or twice to survey it, when all farther notice of it ceases.

The accuracy of this statement may perhaps be questioned. It may be considered as highly improbable, that the capsule should be deposited after this manner. Strange, however, as it may appear, I have several times witnessed the leech drawing the anterior part of its body through it, as through a ring. Indeed, I know of no other way in which it can possibly get rid of it, *the membrane forming a complete band round its body.* Although this mode may be somewhat singular, yet there is little in it to excite our surprise, in comparison with what we find recorded in the works of naturalists.

When deposited in an unattached state, that is, left free, and floating in the water, the capsules are mostly of a globular form (as in Fig. 6). When fixed to any substance, they present an oval form (as in Fig. 7), which is by far the most

common. They are at first of a greyish white colour, *a a*, but in ten or fifteen minutes become of an amber colour, *b b*. They differ much as to size, but are usually about three lines in length, and two in breadth, convex above and flattened beneath. The dark points (Fig. 8, *a a*) mark the openings left in the capsule by the manner in which the leech deposits it, and are those places from which (the resistance there being less than in any other part) the young escape. They are deposited during the whole of the summer months, and even so late as the month of October (29th), and contain each from six to twelve ova; which are well defined as to figure, being perfectly round, and have a smooth uniform appearance. The ova, and the enveloping membrane are represented, magnified, in Fig. 8. In about a fortnight the ova are much increased in size, and show rough edges (Fig. 9.) In three weeks, they take the form of an oblong oval (Fig. 10), when animal existence is, for the first time, developed: which consists, in simply an elongation and contraction of the body. At this period, there is little or no resemblance to the parent animal. In six weeks, the young are completely formed, and in active motion about to quit the capsule. Fig. 11 shows the included young at this period.

It is not a little amusing to witness their exertions to escape from their imprisonment. They contract themselves, as it respects their length, into as small a compass as possible, and then forcibly push forward the head, butting, as it were, at the dark point of the capsule to effect their escape. After many efforts, they succeed in making a small opening, through which they endeavour to force a passage. I have frequently watched them, the head having free motion

without the capsule, using their utmost exertions to free themselves, but not being able to accomplish this, they have returned to their former situation, renewing their efforts occasionally, until their object was attained.

At the time of birth, they are nearly colourless, and continue so for many months, with very little increase as to size. They have the property of moving on the surface of the water, with their belly uppermost. I have noticed nothing of this nature in these animals, when fully grown. According to MULLER, this faculty is possessed by the *Hirudo hippoglossi*: he says, "Præter hunc incessum (more geometrarum) alium in hac specie observavi, inversum nempe, dum corpore supino, summum aquæ ore et cauda, alternatim prehendit." Hist. Verm. ii. p. 51. I have observed it to be also common to the *Hirudo stagnalis* and the *Hirudo complanata*. These animals, differing in several particulars from the leech, are now formed into a distinct genus; to which, from their possessing a retractile tubular tongue, we have given the name of GLOSSIPHONIA.

Considering the quantity of the *H. vulgaris* that I preserved, during the summer, I was surprised at the comparatively small number of capsules deposited. This was at length accounted for. Whilst watching one of them, during the formation of this membrane, I observed another move forward towards the same place, seize it in its mouth, and tear it into three or four shreds, which it left floating in the water.

I now placed the *H. vulgaris*, singly, in different vessels, and found I could by this mode increase the number of the capsules. Each leech produced, in less than a month, from

six to twelve. One of them deposited, from the 8th of October to the 29th of the same month, no less than twelve capsules; and taking an average number of the ova they contain, and supposing only one-third to be productive, we should have thirty-six young from this single leech.

From the similarity of the *Hirudo vulgaris*, both in its structure and general appearance, to the *Hirudo medicinalis*, I think, we have every reason for believing that the latter is also oviparous; and that the ova have an enveloping membrane, which is formed and deposited, after the manner already mentioned.

From what has been advanced, we learn,

1. That the *Hirudo vulgaris* copulates in the same manner as the common snail.

2. That it is oviparous.

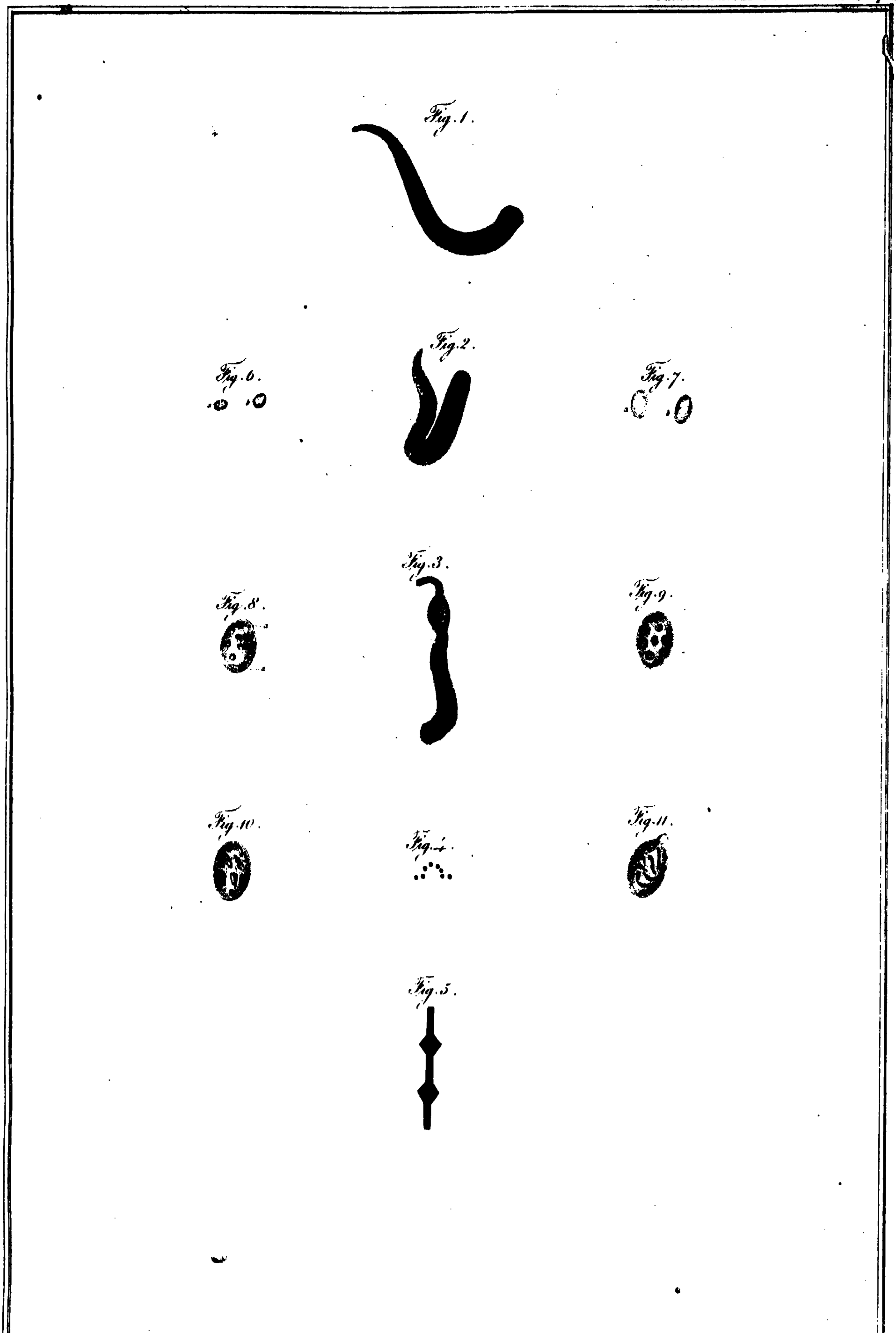
3. That the ova are imbedded in a gelatinous mass enveloped by a strong semi-transparent fibrous membrane, denominated the capsule.

4. That the capsule contains from six to twelve ova, which are globular, and have a smooth surface.

5. That these ova in the course of a fortnight, lose their globular shape, and show jagged edges: that in three weeks, they take the form of an oblong oval, when we discover animal existence: and in six weeks, the young make their escape.

6. That the number of the capsules each leech produces, (one every second or third day) varies from six to twelve.

7. That the young are nearly colourless at the time of birth, and continue so for many months, increasing little as to size.



James Basire sculp.

EXPLANATION OF THE DRAWING.

Fig. 1. A front view of the *Hirudo vulgaris*, of its natural size.

Fig. 2. A back view of the same.

Fig. 3 The appearance it presents previous to its depositing the capsule.

Fig. 4. The eyes (magnified) showing their arrangement.

Fig. 5. A portion of the abdominal blood-vessel, magnified, showing its diamond-figured dilatations.

Fig. 6. The globular form of the capsule, when unattached, (of its natural size).—*a*, its appearance at the moment of deposition: *b*, the appearance it afterwards assumes.

Fig. 7. The form of this membrane (also of its natural size) when attached to any surrounding body.

Fig. 8. The capsule, magnified, showing the included ova, (*a a*) the points from which the young escape.

Fig. 9. The appearance of the ova at the end of a fortnight.

Fig. 10. The ova at the expiration of the third week, when animal existence is first observed.

Fig. 11. The appearance of the young at the termination of the sixth week, when they are about to quit the capsule.

JAMES RAWLINS JOHNSON.

Bristol, October 30, 1816.

III. *On the effects of galvanism in restoring the due action of the lungs.* *By* A. P. Wilson Philip, *Physician in Worcester.* *Communicated by Sir* Everard Home, *Bart. V. P. R. S.*

Read November 21, 1816.

In the prosecution of an inquiry in which I have been engaged for several years, some of the results of which were published in the Philosophical Transactions of last year, I have had occasion to make many experiments with galvanism, which seem to me to point out with more precision than has yet been done, what we are to expect from it in the cure of disease; and I think it will appear from what I am about to say, that to the want of discrimination in its employment we must ascribe the little advantage which medicine has hitherto derived from the discovery of this influence.

It seems to be an inference both from my own experiments and observations and those of others, which I had the honour to lay before the Society in my first paper, that what is called the nervous system, comprehends two distinct systems, the sensorial, and the nervous system properly so called. Now it does not appear that galvanism can perform any of the functions of the sensorial system, yet in the greater number of instances in which it has been used in medicine, it has been expected to restore the sensorial power. It has been expected to restore hearing, and sight, and voluntary power. It may now and then happen in favorable cases, from the connection

which subsists between the sensorial and nervous systems, that by rousing the energy of the latter, we may excite the former. It would be easy to show, that we have little reason to expect that this will often happen. It also appears from the experiments to which I allude, that galvanism has no other power over the muscular system, than that of a stimulus; we are, therefore, to expect little more advantage from it in diseases depending on faults of the sanguiferous system, than from other stimuli. Hence its failure in tumors, &c. But I cannot help regarding it as almost ascertained, that in those diseases in which the derangement is in the nervous power alone, where the sensorial functions are entire, and the vessels healthy, and merely the power of secretion, which seems immediately to depend on the nervous system, is in fault, galvanism will often prove a valuable means of relief.

As soon as this view of the subject presented itself, I was led to inquire, what diseases depend on a failure of nervous influence. The effect on the lungs of dividing the eighth pair of nerves answered the question respecting one of the most important diseases of this class. We find that withdrawing a considerable part of the nervous influence from the lungs, produces great difficulty of breathing. When the effect of this experiment on the lungs is carefully attended to, it will be found, I think, that it is in all respects similar to the disease which may be called habitual asthma; in which the breathing is constantly oppressed, better and worse at different times, but never free, and often continues to get worse in defiance of every means we can employ, till the patient is permanently unfitted for all the active duties of life. The animal in the above experiment is not affected with the

croaking noise and violent agitation which generally characterize fits of spasmodic asthma. This state we cannot induce artificially, except by means which lessen the aperture of the glottis.

I found from repeated trials, that both the oppressed breathing and the collection of phlegm, caused by the division of the eighth pair of nerves, may be prevented by sending a stream of galvanism through the lungs. That this may be done with safety in the human body, we know from numberless instances in which galvanism has been applied to it in every possible way.

Such are the circumstances which led me to expect relief from galvanism in habitual asthma. It is because that expectation has not been disappointed, that I trouble the Society with this Paper. Although the effects of galvanism in habitual asthma have been witnessed by many other medical men, I have mentioned nothing in the following pages which did not come under my own observation.

I have employed galvanism in many cases of habitual asthma, and almost uniformly with relief. The time, during which the galvanism was applied before the patient said that his breathing was easy, has varied from five minutes to a quarter of an hour. I speak of its application in as great a degree as the patient could bear without complaint. For this effect I generally found from eight to sixteen four-inch plates of zinc and copper, the fluid employed being one part of muriatic acid, and twenty of water, sufficient. Some require more than sixteen plates, and a few cannot bear so many as eight; for the sensibility of different individuals to galvanism is very different. It is curious, and not easily accounted for,

that a considerable power, that perhaps of twenty-five or thirty plates is often necessary on first applying the galvanism, in order to excite any sensation; yet after the sensation is once excited, the patient shall not perhaps, particularly at first, be able to bear more than six or eight plates. The stronger the sensation excited, the more speedy in general is the relief. I have known the breathing instantly relieved by a very strong power. I have generally made it a rule to begin with a very weak one, increasing it gradually at the patient's request, by moving one of the wires from one division of the trough to another, and moving it back again when he complained of the sensation being too strong. It is convenient for this purpose to charge with the fluid about thirty plates.

The galvanism was applied in the following manner. Two thin plates of metal about two or three inches in diameter, dipped in water, were applied, one to the nape of the neck, the other to the pit of the stomach, or rather lower. The wires from the different ends of the trough* were brought into contact with these plates, and, as observed above, as great a galvanic power maintained, as the patient could bear without complaint. In this way the galvanic fluid was sent through the lungs, as much as possible in the direction of their nerves. It is proper, constantly to move the wires upon the metal plates, particularly the negative wire, otherwise the cuticle is injured in the place on which it rests. The relief seemed much the same, whether the positive wire was applied to the nape of the neck, or the pit of the stomach. The negative

* I found a trough of the old construction answer better than the improved pile, which is so much superior for most purposes.

wire generally excites the strongest sensation. Some patients thought, that the relief was most speedy, when it was applied near the pit of the stomach.

The galvanism was discontinued, as soon as the patient said that his breathing was easy. In the first cases in which I used it, I sometimes prolonged its application for a quarter of an hour, or twenty minutes, after the patient said he was perfectly relieved, in the hope of preventing the early recurrence of the dyspnœa; but I did not find that it had this effect. It is remarkable, that in several who had laboured under asthmatic breathing for from ten to twenty years, it gave relief quite as readily as in more recent cases; which seems to prove, that the habitual difficulty of breathing, even in the most protracted cases, is not to be ascribed to any permanent change having taken place in the lungs.

With regard to that form of asthma which returns in violent paroxysms, with intervals of perfectly free breathing, I should expect little advantage from galvanism in it, because, as I have just observed, I found that the peculiar difficulty of breathing, which occurs in this species of asthma, cannot be induced in animals, except by means lessening the aperture of the glottis. It is probable, that in the human subject the cause producing this effect is spasm, from which indeed the disease takes its name, and we have no reason to believe, from what we know of the nature of galvanism, that it will be found a means of relaxing spasm.

The spasmodic asthma is fortunately a very rare disease, so much so, that but one case of it has occurred to me since I have employed galvanism in asthma, while I have had an opportunity of employing this remedy in between thirty and

forty cases of the habitual form of the disease. I cannot, therefore, from experience, speak with certainty of the effect of galvanism in the former. In the above case it was twice employed in the paroxysm, and I could observe no relief from it. In both instances, the patient said, that, had it not been used, the symptoms would have been more severe. In this patient, the spasmodic paroxysm was often succeeded by a state of habitual asthma for several weeks, in which galvanism gave immediate, but temporary relief.

Of the above cases of habitual asthma, many occurred in work-people of this town, who had been obliged to abandon their employments in consequence of it, and some of them, from its long continuance, without any hope of returning to regular work. Most of them had tried the usual means in vain. By the use of galvanism they were all restored to their employments. I have seen several of them lately, who, although they have not used the galvanism for some months, said, they had continued to work without any inconvenience. Some, in whom the disease had been wholly removed, remained quite free from it; some have had a return of it, and have derived the same advantage from the galvanism as at first.

I have confined the application of galvanism [illegible] sthmatic dyspnœa. I think there is reason to believe, from the experiments which I have made, that in inflammatory cases it would be injurious, and, in cases arising from dropsy or any other mechanical impediment, little or nothing, it is evident, is to be expected from it. Habitual asthma is often attended with a languid state of the biliary system, and some fullness and tenderness on pressure near the pit of the stomach. If these are considerable, they must be relieved, previous to the use of the galvanism. When there is a considerable tendency

to inflammation, the repeated application of the galvanism sometimes increases it so much, that the use of this influence no longer gives relief, till the inflammatory tendency is subdued by local blood-letting. It always gave relief most readily, and the relief was most permanent in those cases of habitual asthma which were least complicated with other diseases, the chief complaint being a sense of tightness across the region of the stomach, impeding the breathing. The patients said, that the sense of tightness gradually abated, while they were under the influence of the galvanism, and that as this happened their breathing became free. The abatement of the tightness was often attended with a sense of warmth in the stomach, which seemed to come in its place. This sensation was most frequently felt when the negative wire was applied near the pit of the stomach, but the relief did not seem less when it was not felt.

With respect to the continuance of the relief obtained by galvanism, it was different in different cases; in the most severe cases it did not last so long as in those where the symptoms were slighter, though of equal continuance. This observation, however, did not universally apply. When the patient was galvanised in the morning, he generally felt its good effects till next morning. In almost all, the repetition of the galvanism gradually increased the degree of permanent relief. Its increase was much more rapid in some cases than in others. The galvanism was seldom used more than once a day. In some of the more severe cases it was used morning and evening. About a sixth part of those who have used it appear, as far as we yet know, to have obtained a radical cure. It failed to give considerable relief only in about one tenth. I may add, that were it only the means of present

relief, we have reason to believe that, as being more innocent, it would be found preferable to the heating, spirituous, and soporific medicines, which are so constantly employed in this disease.

As it often happened that a very small galvanic power, that of not more than from four to six four-inch double plates, relieved the dyspnœa, may we not hope, that a galvanic apparatus may be constructed, which can be worn by the patient, of sufficient power to prevent its recurrence in some of the cases, in which the occasional use of the remedy does not produce a radical cure?

I wished to try, if the impression on the mind, in the employment of galvanism, had any share in the relief obtained from it. I found, that by scratching the skin with the sharp end of a wire, I could produce a sensation so similar to that excited by galvanism, that those who had most frequently been subjected to this influence were deceived by it. By these means, and arranging the trough, pieces of metal, &c. as usual, I deceived several who had formerly received relief from galvanism, and also several who had not yet used it. All of them said that they experienced no relief from what I did. Without allowing them to rise, I substituted for this process the real application of galvanism, merely by immersing in the trough one end of the wire with which I had scratched the nape of the neck, the wire at the pit of the stomach having been all the time applied as usual by the patient himself. Before the application of the galvanism had been continued as long as the previous process, they all said they were relieved. I relate the particulars of the two following experi-

ments, because, independently of the principal object in view in making them, they point out two circumstances of importance in judging of the *modus operandi* of galvanism in asthmatic cases.

The first was made on an unusually intelligent lady, of about thirty-five years of age, who had for many years laboured under habitual asthma, than whom I have known none more capable of giving a distinct account of their feelings. Her breathing was very much oppressed at the time that she first used galvanism. The immediate effect was, that she breathed with ease. She said, she had not breathed so well for several years. Part of the relief she obtained, proved permanent, and, when she was galvanised once a day for about ten minutes, she suffered little dyspnœa at any time. After she had been galvanised for eight or ten days, I deceived her in the manner just mentioned. The deception was complete. She told me to increase or lessen the force of the galvanism, as she was accustomed to do, according to the sensation it produced. I obeyed her directions by increasing or lessening the force with which I scratched the neck with the wire. After I had done this for five minutes, she said the galvanism did not relieve her as usual, and that she felt the state of her breathing the same as when the operation was begun. I then allowed the galvanism to pass through the chest, but only through the upper part of it, the wire in front being applied about the middle of the sternum. She soon said that she felt a little relief; but although it was continued in this way for ten minutes, the relief was imperfect. I then directed her to apply the wire in front to the pit of the stomach, so that the galvanism passed through the whole

extent of the chest, and, in a minute and a half, she said her breathing was easy, and that she now experienced the whole of the effect of the former applications of the remedy.

To try how far the effect of galvanism in asthma arises merely from its stimulating the spinal marrow, in a young woman who had been several times galvanised in the usual way, and in whom it eventually performed a permanent cure,* the wires were applied to the nape of the neck and small of the back, and thus the galvanic influence was sent along the spine for nearly a quarter of an hour. She said her breathing was easier, but not so much so as on the former applications of the galvanism; and on attempting to walk up stairs she began to pant, and found her breathing, when she had gone about half way, as difficult as before the application of the galvanism. She was then galvanised in the usual way for five minutes: she now said her breathing was quite easy, and she walked up the whole of the stairs without bringing on any degree of panting, or feeling any dyspnœa. This experiment was made in the presence of four medical gentlemen.

Many medical gentlemen have frequently witnessed the relief afforded by galvanism in habitual asthma, and Mr. Cole, the house surgeon of the Worcester Infirmary authorises me to say, that no other means there employed have been equally efficacious in relieving this disease.

* This patient, after remaining free from her disease for about half a year, has, since the above was written, returned to the infirmary, labouring under a slighter degree of it, and again experienced immediate relief from galvanism. The disease seemed to have been renewed by cold, which had at the same time produced other complaints.

Worcester, November 4th, 1816.

IV. *Account of some experiments on the Torpedo electricus, at La Rochelle. By* John T. Todd, *Esq. Communicated by Sir* Everard Home, *Bart. V. P. R. S.*

Read December 5, 1816.

The Royal Society having condescended to accept a Paper which I had the honor of submitting to them, through the kindness of Sir Everard Home, on the subject of the Torpedo electricus, may perhaps, from the same motives, be induced to receive the following commencement of a set of experiments made at La Rochelle, unfortunately interrupted by some untoward circumstances.

The following series had for its principal object, to determine whether the Torpedo possessed any voluntary power over the electrical organs, either in exciting or interrupting their action, except through the nerves of these organs. They were made on board the fishing boats, immediately after the fish were caught.

The two Torpedos, the subjects of my first experiments, were of very different sizes; the one being about eight inches in length, and the other eighteen. They were of the same colour, light hazle grey, and mottled. The shocks were easily excited; those of the larger being much more severe. The fishermen held them by the tail without any apprehension of receiving the shock. They possessed the same security when they held them by that part between the

anterior extremities of the electrical organs. When the torpedos were placed in a bucket of water, the fishermen amused themselves by exposing the smaller fish to their electrical power.

EXPERIMENTS.

I intersected the large lateral cartilages of the smallest torpedo near their posterior connection with the trunk, and all the muscles inserted into them. The shocks continued to be received as before.

I divided in the same torpedo that part extending from the anterior part of the large lateral cartilages to the process projecting from the anterior part of the head. No alteration was observed in the production of the shocks.

The same experiments were repeated on the larger torpedo with the same results.

I removed the superior surface of the right electrical organ of the largest torpedo. Shocks were received as before.

I made a vertical and longitudinal incision two and a half inches in length, in both the electrical organs of the largest torpedo. I received the shocks as before.

The same experiments were repeated on the smallest one with the same results.

The torpedos in the intervals of the experiments were allowed to remain in water, and at this period they were allowed to remain a quarter of an hour. When examined, they seemed considerably exhausted. The smallest one was still capable of producing weak shocks. The largest one was less exhausted.

I continued the above-mentioned incisions in the largest one, so as to remove one half of each electrical organ. Shocks were still received, though weaker.

I repeated the same on the smaller one. The shocks were with difficulty distinguishable. I cannot attribute the weakness of the shocks more to the removal of a part of the organs, than to the exhaustion from repeated action.

The next subject of my experiments was about nine or ten inches in length, and of the same colour as the former ones. It was lively, and parted with its shocks freely. When held by the tail, or that part placed between the anterior extremities of the electrical organs, it was, as before observed, perfectly incapable of communicating the shocks.

I made an incision extending round the circumference of both organs, so as to leave no attachment between the electrical organs and the animal, except by the nerves. Shocks were received as before.

I removed the large lateral cartilages, and denuded a large portion of the surfaces of the electrical organs. After this change, two distinct shocks were received, but the animal being much injured, soon died.

In performing these experiments, I observed how powerfully the action of the electrical organs was excited by the cutting of the scalpel, and on one occasion pressing on the electrical organ with my left hand, and holding the scalpel wet in the other, while cutting the electrical organ, I received a distinct shock in the right hand through the scalpel. In dissecting these animals, I had also the occasion of remarking, that all the nerves of the electrical organs arise from the *medulla oblongata*, notwithstanding the long course which three of them are obliged to follow, before they penetrate the electrical organs.

The torpedo termed " *la tremble*" by the lower orders in

France, is met with in considerable quantities, as has been long known, on the whole extent of coast between the Loire and the Garonne. It is generally caught by the trawl. Though not esteemed, it is yet eaten by the poorer inhabitants, being first skinned and dried. The electrical organs are carefully avoided in eating, being considered to possess some disagreeable properties.

V. *A description of a process, by which corn tainted with Must may be completely purified. By* Charles Hatchett, *Esq. F. R. S. In a Letter addressed to the Right Honourable Sir Joseph Banks, Bart. G. C. B. P. R. S. &c. &c.*

Read December 5, 1816.

MY DEAR SIR,

THE very great loss which this country formerly experienced by a considerable part of imported grain having become contaminated by Must, induced me, several years past, to direct my attention towards discovering some simple and economical method by which this taint could be removed, and you well know that my endeavours were successful; but as circumstances at that time, and since, did not appear to require that great publicity should be given to this process, I contented myself with describing it to you and a few of my other friends. Now, however, when I reflect on the large quantities of corn which, during the last harvest, have been housed in a damp state, and on the great importations which are expected, with the extreme probability that a considerable part may have contracted Must, and that thus the object of importation may be partially frustrated by the destruction of a large portion of grain, and the consequent increase in the price of the remainder, I think it incumbent on me, by addressing this Letter to you, to lose no time in publishing a process, by which corn, however musty, may be completely

purified, with scarcely any loss of quantity, with very little expense, and without requiring previous chemical knowledge or chemical apparatus.

The experiments which I made, were confined to wheat, as being of the greatest importance; but there can be no doubt that oats and other grain may be restored to sweetness with equal success; and I have also additional satisfaction from being enabled to state, that the efficacy of the process may be ascertained by any person, in any place, and upon any quantity of grain, however small.

From my experiments I am inclined to believe, that Must is a taint produced by damp upon the amylaceous part of the grain or starch; that the portion of starch nearest to the husk is that which is first tainted; and that the greater or less degree of Must is in proportion to the taint having penetrated more or less into the substance of the grain. In most cases, however, the taint is only superficial; but nevertheless, if not removed, it is sufficient to contaminate the odour and flavour of the whole, especially when converted into flour.

After various experiments, I found the following method to be attended with success.

The wheat must be put into any convenient vessel capable of containing at least three times the quantity, and the vessel must be subsequently filled with boiling water; the grain should then be occasionally stirred, and the hollow and decayed grains (which will float) may be removed; when the water has become cold, or in general when about half an hour has elapsed, it is to be drawn off. It will be proper then to rince the corn with cold water, in order to remove any portion of the water which had taken up the Must; after which the

corn being completely drained, is without loss of time to be thinly spread on the floor of a kiln, and thoroughly dried, care being taken to stir and to turn it frequently during this part of the process.

This is all that is required; and I have constantly found that even the most musty corn (on which ordinary kiln drying had been tried without effect) thus became completely purified, whilst the diminution of weight caused by the solution of the tainted part was very inconsiderable.

I have the honour to remain,

Dear Sir Joseph,

your most faithful and obedient Servant,

CHARLES HATCHETT.

Mount Clare, Roehampton,
Dec. 4, 1816.

VI. *Observations on an astringent vegetable substance from China.* *By* William Thomas Brande, *Esq. Sec. R. S.*

Read December 12, 1816.

THE substance described in the following pages was put into my hands for examination by the President, who received it from China, with some others employed in the art of dying; and although the small quantity hitherto sent to this country, has not enabled me to extend my experiments upon its useful applications as far as I could have wished, I trust that its chemical history will be deemed of sufficient importance to interest the Royal Society, and to prove its usefulness as an article of commerce, provided it can be obtained abundantly, and at a cheap rate, which I think admits of little doubt.

The parcel containing this substance was marked "Oong poey," *a species of galls used in dying black.* They have the appearance of irregular vesicles, the coats of which are about one-tenth of an inch thick, of a grey and reddish colour, smooth, and very brittle, and containing in their interior a brown powder, among which insects may be discerned by the microscope. Some of these vesicles were adhering to twigs of the tree, and they appear to be formed upon the younger branches.

They have a more austere and purely astringent taste than any other of the vegetable substances of that class I have met with, and they produce, when thrown into any of the red salts of iron, a pure black tint.

Of the source of these bodies nothing is said, but on referring to DU HALDE, (description de l'Empire de la Chine, &c. folio, Paris 1735, page 496.) I found an account of a Chinese drug, entitled *ou poey tse*, and which appears to be the substance in question. Their formation is ascribed to small insects, and the general description of their exterior characters agrees nearly with that I have given: they vary in size, from a small gall nut to a large chesnut. M. GEOFFROY, in the Memoires de l'Académie Royale des Sciences, 1724, has published a Paper entitled, "Observations sur les vessies qui viennent aux Ormes, et sur une sorte d'excroissance à peu près pareille qui nous est apportée de la Chine." He conceives that the excrescences occasionally formed upon the elm are similar to those from China, but does not identify the two by any experiments; and indeed it would appear, from the account given by DU HALDE, that the *ou poey tse* are obtained from a very different tree. These Chinese galls are likewise employed in medicine, and a full account of their various preparations are annexed to DU HALDE's observations.

My first experiments were directed towards ascertaining the quantity of tannin which they contained, and which I have found considerably greater than that in any other vegetable astringent in common use.

One hundred grains of the Chinese galls, freed from extraneous matters, were reduced to a coarse powder, and infused in cold distilled water, till that fluid ceased to act upon the residuum. The infusion was of a very pale brown colour, and of a highly astringent flavour; it furnished a copious white precipitate with a solution of jelly, and became deep black upon the addition of the oxy-sulphate of iron. When

carefully evaporated to dryness, there remained upon the glass capsule 75 grains of a brown transparent substance, having a resinous fracture, a rough, astringent, and slightly sour taste, and which powerfully reddened litmus paper. It was quite soluble in cold water, and the solution had the same properties as the former, except that its colour was somewhat deeper. It was perfectly soluble in alcohol, (sp. gr., 820 at 60°), and its properties were not altered by repeated solution in water and evaporation.

It appears from these characters, that the substance in question contains tannin nearly free from extractive matter. Indeed, I am not aware that tannin exists in the same state of purity in any other vegetable product.

The residuum which had resisted the action of water weighed when dry 27 grains; it was digested in 2 ounces of alcohol, which acquired a slightly brown tint, and was rendered turbid by the addition of water. The substance that precipitated was fusible and inflammable, and had the other characters of resin. When heated it exhaled a very peculiar odour. There now remained 23 grains of a grey substance insoluble in boiling water and alcohol, and which when heated burned quietly away without residuum, and therefore possessed the characters of woody fibre.

During the preceding experiments, several circumstances occurred, which induced me to believe, that the aqueous solution, though remarkably free from extractive matter, contained a considerable proportion of gallic acid, I therefore endeavoured to ascertain the relative quantity of this acid contained in the brown residuum obtained from the watery infusion. For this purpose it was boiled in water with carbonate of barytes,

according to the process recommended by Sir H. DAVY, and the gallate of barytes was subsequently decomposed by dilute sulphuric acid; I found it, however, impossible to obtain the gallic acid in a free state, on account of the facility with which it was decomposed on attempting to evaporate the solution.

When lime water is added to the aqueous infusion of the galls, a copious insoluble precipitate is formed, consisting of tannin and lime, and a gallate of lime remains in solution, which is decomposed by oxalic acid. In this way I succeeded in procuring the gallic acid nearly pure.

I boiled some pure caustic lime in a strong infusion of the galls, and when cold, filtered the mixture: oxalic acid was added as long as it produced a precipitate in the filtered liquor, heat was applied, and after separating the oxalate of lime, a solution of nearly pure gallic acid was obtained.

I have failed in all these experiments in obtaining the gallic acid perfectly pure, but the Chinese galls appear to me to offer a most promising source of that acid in its pure state, and the gallates obtained by the processes above described, seem to be entirely free from extractive matter and to approach nearer to pure salts, than those which are procured from infusions of the common galls.

When the Chinese galls are exposed in a glass retort to the heat of an Argand lamp, a considerable quantity of gallic acid tainted by empyreumatic oil, rises into the neck of the vessel, and if the heat be continued, the water which is produced dissolves it, and carries it over into the receiver: during destructive distillation, therefore, a considerable portion of liquid gallic acid may be thus obtained.

One hundred grains of the galls in powder, were submitted

to the action of heat, gradually raised to redness, in a retort to which a proper apparatus was adopted for collecting the liquid and gaseous products. They afforded the following results.

	Grains.
Water tainted by empyreumatic oil, and holding gallic acid in solution - - - -	50
Gaseous compounds of charcoal with oxygen and hydrogen - - - - - -	10
Charcoal remaining in the retort, and affording traces of minute quantities of lime - -	38
	98

It appears from the foregoing experiments, that the substance existing in the Chinese galls which has the power of forming an insoluble white precipitate with animal jelly, and which has a purely astringent flavour, is also perfectly soluble in alcohol; hence it seems, that the assertion of many chemical writers concerning the insolubility of pure tannin in that menstruum is not correct. In this respect the tannin of the China galls resembles that obtained from catechu, the properties of which have been examined by Sir H. DAVY,* and it is probable, that the tannin described by BOUILLON LA GRANGE † as insoluble in alcohol, obtained from infusion of galls by carbonate of ammonia, was not pure.

The want of extractive matter in the China galls, would probably render them very unfit for the purposes of tanning, and I do not find from DU HALDE, that they are ever applied by the Chinese to that use. I found the leather produced by

* Phil. Trans. 1803.

† Annales de Chimie, Vol. 56.

their infusion extremely brittle when dried. The same circumstance however, namely, the absence of extractive principle, probably would materially contribute to their excellence as a source of the black dye, the intensity and perfection of which is, I conceive, often interfered with by the presence of extractive matter in the common gall nut and other vegetable astringents usually employed. These galls are likewise particularly proper for the production of writing ink, the tendency of which to become thick and mouldy seems principally to be derived from extractive matter.

VII. *Some researches on flame.* *By Sir* Humphry Davy, *LL.D. F.R.S. V.P.R.I.*

Read January 16, 1817.

I HAVE described in three papers which the Royal Society have honoured with a place in their Transactions, a number of experiments on combustion which show that the explosion of gaseous mixtures can be prevented or arrested by various cooling influences, and which led me to discover a tissue permeable to light and air, but impermeable to flame, on which I founded the invention of the wire gauze safe lamp now generally used in all collieries in which inflammable air prevails, for the preservation of the lives and persons of the miners. In a short notice published in the third number of the Journal of Science and the Arts, edited at the Royal Institution, I have given an account of some new results on flame, which show that the intensity of the light of flames depends principally upon the production and ignition of solid matter in combustion, and that the heat and light in this process are in a great measure independent phenomena. Since this notice has been printed, I have made a number of researches on flame: and as they appear to me to throw some new lights on this important subject, and to lead to some practical views connected with the useful arts, I shall without any farther apology, present them to the Royal Society.

That greater distinctness may exist in the details, I shall

treat of my subjects under four heads. In the first I shall discuss the effects of rarefaction, by partly removing the pressure of the atmosphere upon flame and explosion. In the second, I shall consider the effects of heat in combustion. In the third, I shall examine the effect of the mixture of gaseous substances not concerned in combustion upon flame and explosion. In the fourth, I shall offer some general views upon flame, and point out certain practical and theoretical applications of the results.

I. *On the effect of rarefaction by partly removing the pressure of the atmosphere upon flame and explosion.*

The earlier experimenters upon the BOYLEAN vacuum observed that flame ceased in highly rarefied air: but the degree of rarefaction necessary for this effect, has been differently stated. Amongst late experimenters, M. de GROTTHUS has examined this subject. He has asserted that a mixture of oxygene and hydrogene ceases to be explosive by the electrical spark when rarefied sixteen times, and that a mixture of chlorine and hydrogene cannot be exploded when rarefied only six times, and he generalises by supposing that rarefaction, whether produced by removing pressure or by heat, has the same effect.

I shall not begin by discussing the experiments of this ingenious author. My own results and conclusions are very different from his; and the cause of this difference, will I think be obvious in the course of these inquiries. I shall proceed in stating the observations which guided my researches.

When hydrogene gas slowly produced from a proper mixture was inflamed at a fine orifice of a glass tube, as in the

experiment called the philosophical candle, so as to make a jet of flame of about $\frac{1}{6}$ of an inch in height, and introduced under the receiver of an air pump containing from 200 to 300 cubical inches of air, the flame enlarged as the receiver became exhausted; and when the gage indicated a pressure between 4 and 5 times less than that of the atmosphere was at its maximum of size, it then gradually diminished below, but burned above till the pressure was between 7 and 8 times less, when it became extinguished.

To ascertain whether the effect depended upon the deficiency of oxygene, I used a larger jet with the same apparatus, when the flame to my surprise burned longer, and when the atmosphere was rarefied ten times, and this in repeated trials. When the larger jet was used, the point of the glass tube became white hot, and continued red hot till the flame was extinguished. It immediately occurred to me, that the heat communicated to the gas by this tube, was the cause that the combustion continued longer in the last trials when the larger flame was used; and the following experiments confirmed the conclusion. A piece of wire of platinum was coiled round the top of the tube, so as to reach into and above the flame. The jet of gas of $\frac{1}{6}$ of an inch in height was lighted, and the exhaustion made; the wire of platinum soon became white hot in the centre of the flame, and a small point of wire near the top fused: it continued white hot till the pressure was 6 times less, when it was 10 times it continued red hot at the upper part, and, as long as it was dull red, the gas though extinguished below, continued to burn in contact with the hot wire, and the combustion did not cease until the pressure was reduced 13 times.

It appears from this result, that the flame of hydrogene is

extinguished in rarefied atmospheres, only when the heat it produces is insufficient to keep up the combustion, which appears to be when it is incapable of communicating visible ignition to metal, and as this is the temperature required for the inflammation of hydrogene at common pressures, it appears that its *combustibility* is neither diminished nor increased by rarefaction from the removal of pressure.

According to this view with respect to hydrogene, it should follow that amongst other combustible bodies, those which require least heat for their combustion, ought to burn in more rarefied air than those that require more heat, and those that produce much heat in their combustion ought to burn, other circumstances being the same, in more rarefied air than those that produce little heat: and every experiment I have made confirms these conclusions. Thus olefiant gas which approaches nearly to hydrogene in the heat produced by its combustion, and which does not require a much higher temperature for its inflammation, when its flame was made by a jet of gas from a bladder connected with a small tube furnished with a wire of platinum, under the same circumstances as hydrogene, ceased to burn when the pressure was diminished between 10 and 11 times: and the flames of alcohol and of the wax taper which require a greater consumption of heat for the volatilization and decomposition of their combustible matter, were extinguished when the pressure was 5 or 6 times less without the wire of platinum, and 7 or 8 times less when the wire was kept in the flame. Light carburetted hydrogene, which produces, as will be seen hereafter, less heat in combustion than any of the common combustible gases, except carbonic oxide, and which requires a higher temperature for its inflammation than any other, had

its flame extinguished, even though the tube was furnished with the wire when the pressure was below $\frac{1}{4}$.

The flame of carbonic oxide which, though it produces little heat in combustion, is as inflammable as hydrogene, burned when the wire was used, the pressure being $\frac{1}{6}$.

The flame of sulphuretted hydrogene, the heat of which is in some measure carried off by the sulphur produced by its decomposition during its combustion in rare air, when burned in the same apparatus as the olefiant and other gases, was extinguished when the pressure was $\frac{1}{7}$.

Sulphur, which requires a lower temperature for its combustion than any common inflammable substance, except phosphorus, burned with a very feeble blue flame in air rarefied fifteen times, and at this pressure the flame heated a wire of platinum to dull redness, nor was it extinguished till the pressure was reduced to $\frac{1}{20}$.*

Phosphorus, as has been shown by M. VAN MARUM, burns in an atmosphere rarefied 60 times; and I found that phosphuretted hydrogene produced a flash of light when admitted into the best vacuum that could be made, by an excellent pump of NAIRN's construction.

The mixture of chlorine and hydrogene inflames at a much lower temperature than that of hydrogene and oxygene, and produces a considerable degree of heat in combustion; it was

* The temperature of the atmosphere diminishes in a certain ratio with its height, which must be attended to in the conclusions respecting combustion in the upper regions of the atmosphere, and the elevation must be somewhat lower than in arithmetical progression, the pressure decreasing in geometrical progression.

There is, however, every reason to believe, that the taper would be extinguished at a height of between 9 and 10 miles, hydrogene between 12 and 13, and sulphur between 15 and 16.

therefore probable that it would bear a greater degree of rarefaction, without having its power of exploding destroyed; and this I found in many trials is actually the case, contrary to the assertion of M. de Grotthus. Oxygene and hydrogene in the proportion to form water, will not explode by the electrical spark when rarefied eighteen times, but hydrogene and chlorine in the proportion to form muriatic acid gas, gave a distinct flash of light under the same circumstances, and they combined with visible inflammation when the spark was passed through them, the exhaustion being to $\frac{1}{24}$th.

The experiment on the flame of hydrogene with the wire of platinum, and which holds good with the flames of the other gases, shows, that by preserving heat in rarefied air, or giving heat to a mixture, inflammation may be continued when, under common circumstances, it would be extinguished. This I found was the case in other instances, when the heat was differently communicated: thus, when camphor was burned in a glass tube, so as to make the upper part of the tube red hot, the inflammation continued when the rarefaction was 9 times, whereas it would only continue in air rarefied 6 times, when it was burned in a thick metallic tube which could not be considerably heated by it.

By bringing a little naphtha in contact with a red hot iron, it produced a faint lambent flame, when there remained in the receiver only $\frac{1}{30}$ of the original quantity of air, though without foreign heat its flame was extinguished when the quantity was $\frac{1}{6}$.

I rarefied a mixture of oxygene and hydrogene by the air pump to about eighteen times, when it could not be inflamed by the electric spark. I then heated strongly the

upper part of the tube till the glass began to soften; and passed the spark, when a feeble flash was observed not reaching far into the tube, the heated gases only appearing to enter into inflammation. This last experiment requires considerable care. If the exhaustion is much greater, or if the heat is raised very slowly,* it does not succeed; and if the heat is raised so high as to make the glass luminous, the flash of light, which is extremely feeble, is not visible: it is difficult to procure the proper degree of exhaustion, and to give the exact degree of heat; I have, however, succeeded three times in obtaining the results, and in one instance it was witnessed by Mr. BRANDE.

To elucidate the enquiry still farther, I made a series of experiments on the heat produced by some of the inflammable gases in combustion. In comparing the heat communicated to wires of platinum by flames of the same size, it was evident, that hydrogene and olefiant gas in oxygene, and hydrogene in chlorine, produced a much greater intensity of heat in combustion, than the other gaseous substances I have named burned in oxygene: but no regular scale could be formed from observations of this kind. I endeavoured to gain some approximations on the subject by burning equal quantities of different gases under the same circumstances, and applying the heat to an apparatus by which it could be measured. For this purpose a mercurial gas holder was furnished with a system of stop cocks, terminating in a strong tube of platinum having a minute aperture. Above this was fixed a copper cup filled with olive oil, in which a thermometer was placed. The oil was heated to 212° to prevent any differences in the communication of heat by the condensation of aqueous vapour;

* The reason will be obvious from what is stated in page 55.

the pressure was the same for the different gases, and they were consumed as nearly as possible in the same time, and the flame applied to the same point of the copper cup, the bottom of which was wiped after each experiment.

The results were as follows:

The flame from olefiant gas raised the thermometer to		270°
———	hydrogene	238
———	sulphuretted hydrogene	232
———	coal gas	236
———	gaseous oxide of carbon	218

The quantities of oxygene consumed (that absorbed by the hydrogene being taken as 1) would be, supposing the combustion perfect, for the olefiant gas 6, for the sulphuretted hydrogene 3, for the carbonic oxide 1. The coal gas contained only a very small proportion of olefiant gas; supposing it to be pure carburetted hydrogene, it would have consumed 4 of oxygene. Taking the elevations of temperature, and the quantities of oxygene consumed as the data, the ratios of the heat produced by the combustion of the different gases, would be for hydrogene 26, for olefiant gas 9.66, for sulphuretted hydrogene 6.66, for carburetted hydrogene 6, for carbonic oxide 6*.

It will be useless to reason upon this ratio as exact, for charcoal was deposited both from the olefiant gas and coal gas during the experiment, and much sulphur was deposited from the sulphuretted hydrogene; and there is great reason to believe, that the capacities of fluids for heat increase with their temperature. It confirms, however, the general con-

* These results may be compared with Mr. Dalton's new System of Chemical Philosophy; they agree in showing that hydrogene produces more heat in combustion than any of its compounds.

clusions, and proves that hydrogene stands at the head of the scale, and gaseous oxide of carbon at the bottom. It might at first view be imagined that, according to this scale, the flame of carbonic oxide ought to be extinguished by rarefaction, at the same degree as that of carburetted hydrogene; but it must be remembered, as I have mentioned in another place, that carbonic oxide is a much more combustible gas. Carbonic oxide inflames in the atmosphere when brought into contact with an iron wire heated to dull redness, whereas carburetted hydrogene is not inflammable by a similar wire, unless it is heated to whiteness so as to burn with sparks.

II. *On the effects of rarefaction by heat on combustion and explosion.*

The results detailed in the preceding section are indirectly opposed to the opinion of M. de Grotthus, that rarefaction by heat destroys the combustibility of gaseous mixtures. Before I made any direct experiments on this subject, I endeavoured to ascertain the degree of expansion which can be communicated to elastic fluids by the strongest heat that can be applied to glass vessels. For this purpose I introduced into a graduated curved glass tube some fusible metal. I heated the fusible metal and the portion of the tube containing the air included by it, under boiling water for some time. I then placed the apparatus in a charcoal fire, and very gradually raised the temperature till the fusible metal appeared luminous when viewed in the shade. At this time the air had expanded so as to occupy 2.25 parts in the tube, it being 1 at the temperature of boiling water. Another experiment was made in a thicker glass tube, and the heat was raised until the tube

began to run together; but though this heat appeared cherry red, the expansion was not to more than 2.5, and a part of this might perhaps have been apparent only, owing to the collapsing of the glass tube before it actually melted. It may be supposed that the oxidation of the fusible metal may have had some effect in making the expansion appear less; but in the first experiment the air was gradually brought back to its original temperature of boiling water, when the absorption was scarcely sensible. If M. GAY LUSSAC's conclusions be taken as the ground work of calculation, and it be supposed that air expands equally for equal increments of temperature, it would appear that the temperature of air capable of rendering glass luminous must be 1035° Fahrenheit.*

M. DE GROTTHUS describes an experiment in which atmospheric air and hydrogene, expanded to four times their bulk over mercury by heat, would not inflame by the electric spark. It is evident, that in this experiment a large quantity of steam or of mercurial vapour must have been present, which, like other inexplosive elastic fluids, prevents combustion when mixed in certain quantities with explosive mixtures; but though he seems aware that his gases were not dry, yet he draws his general conclusion, that expansion by heat destroys the explosive powers of gases, principally from this inconclusive experiment.

I introduced into a small graduated tube over well boiled mercury, a mixture of two parts of hydrogene and one of

* The mode of ascertaining temperatures as high as the point of fusion of glass by the expansion of air, seems more unexceptionable than any other. It gives for the point of visible ignition nearly the same degree as that deduced by NEWTON from the times of the cooling of ignited metal in the atmosphere.

oxygene, and heated the tube by a large spirit lamp till the volume of the gas was increased from 1 to 2.5. I then, by means of a blow pipe and another spirit lamp, made the upper part of the tube red hot, when an explosion instantly took place.

I introduced into a bladder a mixture of oxygene and hydrogene, and connected this bladder with a thick glass tube of about $\frac{1}{8}$ of an inch in diameter and three feet long, curved so that it could be gradually heated in a charcoal furnace; two spirit lamps were placed under the tube where it entered the charcoal fire, and the mixture was very slowly pressed through: an explosion took place before the tube was red hot.

This experiment shows that expansion by heat, instead of diminishing the combustibility of gases, on the contrary, enables them to explode apparently at a lower temperature, which seems perfectly reasonable, as a part of the heat communicated by any ignited body must be lost in gradually raising the temperature. I made several other experiments which establish the same conclusions. A mixture of common air and hydrogene was introduced into a small copper tube, having a stopper not quite tight; the copper tube was placed in a charcoal fire; before it became visibly red an explosion took place, and the stopper was driven out.

I made various experiments on explosions by passing mixtures of hydrogene and oxygene through heated tubes; in the beginning of one of these trials, in which the heat was much below redness, steam appeared to be formed without any combustion. This led me to expose mixtures of oxygene and hydrogene in tubes, in which they were confined by fluid

fusible metal to heat; and I found that by carefully applying a heat between the boiling point of mercury, which is not sufficient for the effect, and a heat approaching to the greatest heat that can be given without making glass luminous in darkness, the combination was effected without any violence, and without any light: and commencing with 212°, the volume of steam formed at the point of combination appeared exactly equal to that of the original gases. So that the first effect in experiments of this kind is an expansion, afterwards a contraction, and then the restoration of the primitive volume.

If when this change is going on, the heat be quickly raised to redness, an explosion takes place; but with small quantities of gas the change is completed in less than a minute.

It is probable, that the slow combination without combustion, already long ago observed with respect to hydrogene and chlorine, oxygene and metals, will happen at certain temperatures with most substances that unite by heat. On trying charcoal, I found that at a temperature which appeared to be a little above the boiling point of quicksilver, it converted oxygene pretty rapidly into carbonic acid, without any luminous appearance, and at a dull red heat, the elements of olefiant gas combined in a similar manner with oxygene, slowly and without explosion.

The effect of the slow combination of oxygene and hydrogene is not connected with their rarefaction by heat, for I found that it took place when the gases were confined in a tube by fusible metal rendered solid at its upper surface; and certainly as rapidly, and without any appearance of light.

M. DE GROTTHUS has stated, that, if a glowing coal be

brought into contact with a mixture of oxygene and hydrogene, it only rarefies them, but does not explode them; but this depends upon the degree of heat communicated by the coal: if it is red in day light and free from ashes, it uniformly explodes the mixture; if its redness is barely visible in shade it will not explode them, but cause their slow combination: and the general phenomenon is wholly unconnected with rarefaction, as is shown by the following circumstance. When the heat is greatest, and before the invisible combination is compleated, if an iron wire heated to whiteness be placed upon the coal within the vessel, the mixture instantly explodes.

Light carburetted hydrogene, or pure fire-damp, as has been shown, requires a very strong heat for its inflammation; it therefore offered a good substance for an experiment on the effect of high degrees of rarefaction by heat on combustion. I mixed together one part of this gas and eight parts of air, and introduced them into a bladder furnished with a capillary tube. I heated this tube till it began to melt, and then slowly passed the mixture through it into the flame of a spirit lamp, when it took fire and burned with its own peculiar explosive light beyond the flame of the lamp, and when withdrawn, though the aperture was quite white hot, it continued to burn vividly.

That the compression in one part of an explosive mixture produced by the sudden expansion of another part by heat, or the elestric spark, is not the cause of combination, as has been supposed by Dr. HIGGINS, M. BERTHOLLET, and others, appears to be evident from what has been stated, and it is rendered still more so by the following facts. A mixture of

hydro-phosphoric gas (bi-phosphuretted hydrogene gas) and oxygene, which explode at a heat a little above that of boiling water, was confined by mercury, and very gradually heated on a sand bath: when the temperature of the mercury was 242°, the mixture exploded.

A similar mixture was placed in a receiver communicating with a condensing syringe, and condensed over mercury till it occupied only $\frac{1}{5}$ of its original volume. No explosion took place, and no chemical change had occurred, for when its volume was restored, it was instantly exploded by the spirit lamp.

It would appear, then, that *the heat* given out by the compression of gases is the real cause of the combustion which it produces, and that at certain elevations of temperature, whether in rarefied or compressed atmospheres, explosion or combustion occurs, i. e. bodies combine with the production of heat and light.

III. *On the effects of the mixture of different gases in explosion and combustion.*

In my first Paper on the fire-damp of coal mines, I have mentioned that carbonic acid gas has a greater power of destroying the explosive power of mixtures of fire-damp and air than azote, and I have ventured to suppose the cause to be its greater density and capacity for heat, in consequence of which it might exert a greater cooling agency, and prevent the temperature of the mixture from being raised to that degree necessary for combustion. I have lately made a series of experiments with the view of determining how far this idea

is correct, and for the purpose of ascertaining the general phenomena of the effects of the mixture of gaseous substances upon explosion and combustion.

I took given volumes of a mixture of two parts of hydrogene and one part of oxygene by measure, and diluting them with various quantities of different elastic fluids, I ascertained at what degree of dilution the power of inflammation by a strong spark from a Leyden phial was destroyed. I found that for one of the mixture inflammation was prevented by

Of Hydrogene, about - -	8
Oxygene - - -	9
Nitrous oxide - -	11
Carburetted hydrogene -	1
Sulphuretted hydrogene -	2
Olefiant gas - - -	$\frac{1}{2}$
Muriatic acid gas - -	2
Silicated fluoric acid gas -	$\frac{5}{6}$

Inflammation took place when the mixtures contained of

Hydrogene - - -	6
Oxygene - - -	7
Nitrous oxide - - -	10
Carburetted hydrogene -	$\frac{3}{4}$
Olefiant gas - - -	$\frac{1}{3}$
Sulphuretted hydrogene -	$1\frac{1}{2}$
Muriatic acid gas - -	$1\frac{1}{2}$
Fluoric acid gas - - -	$\frac{3}{4}$

I hope to be able to repeat these experiments with more precision at no distant time; the results are not sufficiently exact to lay the foundation for any calculations on the relative cooling powers of equal volumes of the gases, but they

show sufficiently, if the conclusions of M. M. DE LA ROCHE and BERARD be correct, that other causes, besides density and capacity for heat, interfere with the phenomena. Thus nitrous oxide, which is nearly $\frac{1}{3}$ denser than oxygene, and which, according to DE LA ROCHE and BERARD, has a greater capacity for heat in the ratio of 1.3503 to .9765 in volume, has lower powers of preventing explosion; and hydrogene, which is 15 times lighter than oxygene, and which in equal volumes has a smaller capacity for heat, certainly has a higher power of preventing explosion; and olefiant gas exceeds all other gaseous substances in a much higher ratio than could have been expected from its density and capacity. The olefiant gas I used was recently made, and might have contained some vapour of ether, and the nitrous oxide was mixed with some azote, but these slight causes could not have interfered with the results to any considerable extent.

Mr. LESLIE, in his elaborate and ingenious researches on heat, has observed the high powers of hydrogene of abstracting heat from solid bodies, as compared with that of common air and oxygene. I made a few experiments on the comparison of the powers of hydrogene, in this respect, with those of carburetted hydrogene, azote, oxygene, olefiant gas, nitrous oxide, chlorine, and carbonic acid gas. The same thermometer raised to the same temperature, 160°, was exposed to equal volumes (21 cubic inches) of olefiant gas, coal gas, carbonic acid gas, chlorine, nitrous oxide gas, hydrogene, oxygene, azote, and air, at equal temperatures, 52° Fahrenheit.

The times required for cooling to 106° were for

	′	″
Air - - - -	2	
Hydrogene - - -		45

	′ ″
Olefiant gas - - -	1.15
Coal gas - -	55
Azote - - -	1.30
Oxygene - -	1.47
*Nitrous oxide - -	2.30. 2.53
*Carbonic acid gas -	2.45
Chlorine - - -	3.6

It appears from these experiments, that the powers of elastic fluids to abstract or conduct away heat from solid surfaces, is in some inverse ratio to their density, and that there is something in the constitution of the light gases, which enables them to carry off heat from solid surfaces in a different manner from that in which they would abstract it in gaseous mixtures, depending probably upon the mobility of their parts.† The heating of gaseous media by the contact of fluid or solid bodies, as has been shown by Count RUMFORD, depends principally upon the change of place of their particles; and it is evident from the results stated in the beginning of this section, that these particles have different powers of abstracting heat analogous to the different powers of solids and fluids. Where an elastic fluid exerts a cooling influence on a solid surface, the effect must depend principally upon the rapidity with which its particles change their places: but where the cooling particles are mixed throughout a mass with other gaseous particles, their effect must princi-

* These two last results were observed by Mr. FARADAY of the Royal Institution, (from whom I receive much useful assistance in most of my experiments), when I was absent from the Laboratory.

† Those particles which are lighest must be conceived most capable of changing place, and would therefore cool solid surfaces most rapidly: in the cooling of gaseous mixtures, the mobility of the particles can be of little consequence.

pally depend upon the power they possess of rapidly abstracting heat from the contiguous particles; and this will depend probably upon two causes, the simple abstracting power by which they become quickly heated, and their capacity for heat which is great in proportion as their temperatures are less raised by this abstraction.

Whatever be the cause of the different cooling powers of the different elastic fluids in preventing inflammation, very simple experiments show that they operate uniformly with respect to the different species of combustion, and that those explosive mixtures, or inflammable bodies, which require least heat for their combustion, require larger quantities of the different gases to prevent the effect, and *vicè versa*; thus one of chlorine and one of hydrogene still inflame when mixed with eighteen times their bulk of oxygene, whereas a mixture of carburetted hydrogene and oxygene in the proper proportions for combinations, one and two, have their inflammation prevented by less than three times their volume of oxygene.

A wax taper was instantly extinguished in air mixed with $\frac{1}{10}$ of silicated fluoric acid gas, and in air mixed with $\frac{1}{6}$ of muriatic acid gas; but the flame of hydrogene burned readily in those mixtures, and in mixtures in which the flame of hydrogene was extinguished, the flame of sulphur burned.

There is a very simple experiment which demonstrates in an elegant manner this general principle. Into a long bottle with a narrow neck introduce a lighted taper, and let it burn till it is extinguished; carefully stop the bottle, and introduce another lighted taper, it will be extinguished before it reaches the bottom of the neck: then introduce a small tube containing zinc and diluted sulphuric acid, and at the aperture of

which the hydrogene is inflamed; the hydrogene will be found to burn in whatever part of the bottle the tube is placed: after the hydrogene is extinguished, introduce lighted sulphur; this will burn for some time, and after its extinction, phosphorus will be as luminous as in the air, and, if heated in the bottle, will produce a pale yellow flame of considerable density.

In cases when the heat required for chemical union is very small, as in the instance of hydrogene and chlorine, a mixture which prevents inflammation will not prevent combination, i. e. the gases will combine without any flash. This I witnessed in mixing two volumes of carburetted hydrogene with one of chlorine and hydrogene; muriatic acid was formed throughout the mixture, and heat produced, as was evident from the expansion when the spark passed, and the rapid contraction afterwards, but the heat was so quickly carried off by the quantity of carburetted hydrogene that no flash was visible.

In the case of phosphorus, which is combustible at the lowest temperature of the atmosphere, no known admixture of elastic fluid prevents the luminous appearance; but this seems to depend upon the light being limited to the solid particles of phosphoric acid formed; whereas to produce flame, a certain mass of elastic fluid must be luminous; and there is every reason to believe, that when phosphuretted hydrogene explodes in very rare air, it is only the phosphorus which is consumed. Any other substance that produces solid matter in combustion would probably be luminous in air as rare, or in mixtures as diluted, as phosphorus, provided the heat was elevated sufficiently for its combustion. I have found that

this is actually the case with respect to zinc. I threw some zinc filings into an ignited iron crucible fixed on the stand of an air pump under a receiver, and exhausted until only $\frac{1}{60}$ of the original quantity of air remained. When I judged that the red hot crucible must be full of the vapour of zinc, I admitted about $\frac{1}{60}$ more of air, when a bright flash of light took place in and above the crucible, similar to that which is produced by admitting air to the vapour of phosphorus in vacuo.

The cooling power of mixtures of elastic fluids in preventing combustion must increase with their condensation, and diminish with their rarefaction; at the same time, the quantity of matter entering into combustion in given spaces, is relatively increased and diminished. The experiments on flame in rarefied atmospherical air, show that the quantity of heat produced in combustion is very slowly diminished by rarefaction, the diminution of the cooling power of the azote being apparently in a higher ratio than the diminution of the heating powers of the burning bodies. I endeavoured to ascertain what would be the effect of condensation on flame in atmospheric air, and whether the cooling power of the azote would increase in a lower ratio, as might be expected, than the heat produced by the increase of the quantity of matter entering into combustion, but I found considerable difficulties in making the experiments with precision. I ascertained, however, that both the light and heat of the flames of the taper, of sulphur and hydrogene, were increased by acting on them by air condensed four times; but not more than they would have been by an addition of $\frac{1}{5}$ of oxygene.

I condensed air nearly five times, and ignited iron wire to

whiteness in it by the voltaic apparatus, but the combustion took place with very little more brightness than in the common atmosphere, and would not continue as in oxygene, nor did charcoal burn much more brightly in this compressed air than in common air. I intend to repeat these experiments, if possible, with higher condensing powers: they show sufficiently that, (for certain limits at least) as rarefaction does not diminish considerably the heat of flame in atmospherical air, so neither does condensation considerably increase it; a circumstance of great importance in the constitution of our atmosphere, which at all the heights or depths at which man can exist, still preserves the same relations to combustion.

It may be concluded from the general law, that at high temperatures, gases not concerned in combustion will have less powers of preventing that operation, and likewise, that steam and vapours, which require a considerable heat for their formation, will have less effect in preventing combustion, particularly of those bodies requiring low temperatures, than gases at the common heat of the atmosphere.

I have made some experiments on the effects of steam, and their results were conformable to these views. I found that a very large quantity of steam was necessary to prevent sulphur from burning. Oxygene and hydrogene exploded by the electric spark when mixed with five times their volume of steam; and even a mixture of air and carburetted hydrogene gas, the least explosive of all mixtures, required a third of steam to prevent its explosion, whereas $\frac{1}{6}$ of azote produced the effect. These trials were made over mercury; heat was applied to water above the mercury, and 37.5 for 100 parts was regarded as the correction for the expansion of the gases.

It is probable that with certain heated mixtures of gases, where the non-supporting or non-inflammable elastic fluids are in great quantities, combination with oxygene will take place, as in the instance mentioned, page 63, of hydrogene and chlorine, without any light, for the temperature produced will not be sufficient to render elastic media luminous; and there are no combustions, except those of the compounds of phosphorus and the metals, in which solid matters are the result of combinations with oxygene. I have shown in the paper referred to in the introduction, that the light of common flames depends almost entirely upon the deposition, ignition and combustion of solid charcoal; but to produce this deposition from gaseous substances demands a high temperature. Phosphorus, which rises in vapour at common temperatures, and the vapour of which combines with oxygene at those temperatures, as I have mentioned before, is always luminous, for each particle of acid formed must, there is every reason to believe, be white hot; but so few of these particles exist in a given space that they scarcely raise the temperature of a solid body exposed to them, though, as in the rapid combustion of phosphorus, where immense numbers are existing in a small space, they produce a most intense heat.

In all cases the quantity of heat communicated by combustion, will be in proportion to the quantity of burning matter coming in contact with the body to be heated. Thus, the blow-pipe and currents of air operate. In the atmosphere, the effect is impeded by the mixture of azote, though still it is very great: with pure oxygene compression produces an immense effect, and with currents of oxygene and hydrogene, there is every reason to believe, that solid matters are made to attain the temperature of the flame. This temperature,

however, evidently presents the limit to experiments of this kind, for bodies exposed to flame can never be hotter than flame itself; whereas in the voltaic apparatus there seems to be no limit to the heat, except the volatilization of the conductors.

The temperatures of flames are probably very different. Where, in chemical changes, there is no change of volume, as in the instance of the mutual action of chlorine and hydrogene, prussic gas (cyanogen) and oxygene, approximations to their temperatures may be gained from the expansion in explosion.

I have made some experiments of this kind by detonating the gases by the electrical spark in a curved tube containing mercury or water; and I judged of the expansion from the quantity of fluid thrown out of the tube: the resistance opposed by mercury, and its great cooling powers, rendered the results very unsatisfactory in the cases in which it was used; but with water, cyanogen and oxygene being employed, they were more conclusive. Cyanogen and oxygene, in the proportion of one to two, detonated in a tube of about $\frac{2}{5}$ of an inch in diameter, displaced a quantity of water which demonstrated an expansion of fifteen times their original bulk. This would indicate a temperature of above 5000° of Fahrenheit, and the real temperature is probably much higher; for heat must be lost by communication to the tube and the water. The heat of the gaseous carbon in combustion in this gas, appears more intense than that of hydrogene; for I found a filament of platinum was fused by a flame of cyanogen in the air which was not fused by a similar flame of hydrogene.

IV. *Some general observations, and practical inferences.*

The knowledge of the cooling power of elastic media in preventing the explosion of the fire-damp, led me to those practical researches which terminated in the discovery of the wire-gauze safe-lamp; and the general investigations of the relation and extent of these powers, serves to elucidate the operation of wire-gauze and other tissues or systems of apertures permeable to light and air, in intercepting flame, and confirms the views I originally gave of the phenomenon.

Flame is gaseous matter heated so highly as to be luminous, and that to a degree of temperature beyond the white heat of solid bodies, as is shown by the circumstance, that air not luminous will communicate this degree of heat.* When an attempt is made to pass flame through a very fine mesh of wire-gauze at the common temperature, the gauze cools each portion of the elastic matter that passes through it, so as to reduce its temperature below that degree at which it is luminous, and the diminution of temperature must be proportional to the smallness of the mesh and the mass of the metal. The power of a metallic or other tissue to prevent explosion, will depend upon the heat required to produce the combustion as compared with that acquired by the tissue; and the flame of the most inflammable substances, and of those that produce most heat in combustion, will pass through a metallic tissue that will interrupt the flame of less inflammable substances, or those that produce little heat in combustion. Or the tissue

* This is proved by the simple experiment of holding a fine wire of platinum about the $\frac{1}{20}$ of an inch from the exterior of the middle of the flame of a spirit lamp, and concealing the flame by an opaque body, the wire will become white hot in a space where there is no visible light.

being the same, and impermeable to all flames at common temperatures, the flames of the most combustible substances, and of those which produce most heat, will most readily pass through it when it is heated, and each will pass through it at a different degree of temperature. In short, all the circum stances which apply to the effect of cooling mixtures upon flame, will apply to cooling perforated surfaces. Thus, the flame of phosphuretted hydrogene at common temperatures, will pass through a tissue sufficiently large not to be immediately choaked up by the phosphoric acid formed, and the phosphorus deposited.* A tissue of 100 apertures to the square inch, made of wire of $\frac{1}{60}$, will at common temperatures intercept the flame of a spirit lamp, but not that of hydrogene; and when strongly heated, it will no longer arrest the flame of the spirit lamp. A tissue which will not interrupt the flame of hydrogene when red hot, will still intercept that of olefiant gas, and a heated tissue which would communicate explosion from a mixture of olefiant gas and air, will stop an explosion from a mixture of fire-damp, or carburetted hydrogene.

The ratio of the combustibility of the different gaseous matters are likewise to a certain extent as the masses of heated matter required to inflame them.† Thus an iron

* If a tissue containing above 700 apertures to the square inch be held over the flame of phosphorus or phosphuretted hydrogene, it does not transmit the flame till it is sufficiently heated to enable the phosphorus to pass through it in vapour. Phosphuretted hydrogene is decomposed in flame, and acts exactly like phosphorus.

† It appeared to me in these experiments, that the worst conducting and best radiating substances required to be heated higher for equal masses to produce the same effect upon the gases; thus, red hot charcoal had evidently less power of inflammation than red hot iron.

wire of $\frac{1}{40}$ of an inch heated cherry red, will not inflame olefiant gas, but it will inflame hydrogene gas; and a wire of $\frac{1}{8}$, heated to the same degree, will inflame olefiant gas; but a wire of $\frac{1}{500}$ must be heated to whiteness to inflame hydrogene, though at a low red heat it will inflame bi-phosphuretted gas; but wire of $\frac{1}{40}$ heated even to whiteness will not inflame mixtures of fire-damp.

These circumstances will explain, why a mesh of wire so much finer is required to prevent the explosion from hydrogene and oxygene from passing, and why so coarse a texture and wire is sufficient to prevent the explosion of the fire-damp, fortunately the least combustible of the known inflammable gases.

The general doctrine of the operation of wire-gauze cannot be better elucidated than in its effects upon the flame of sulphur. When wire-gauze of 600 or 700 apertures to the square inch is held over the flame, fumes of condensed sulphur immediately come through it, and the flame is intercepted; the fumes continue for some instants, but as the heat increases they diminish, and at the moment they disappear, which is long before the gauze becomes red hot, the flame passes; the temperature at which sulphur burns being that at which it is gaseous.

Another very simple illustration of the truth of this view is offered in the effect of the cooling agency of metallic surfaces upon very small flames. Let the smallest possible flame be made by a single thread of cotton immersed in oil, and burning immediately upon the surface of the oil: it will be found to be about $\frac{1}{30}$ of an inch in diameter. Let a fine iron wire of $\frac{1}{180}$ be made into a circle of $\frac{1}{10}$ of an inch in diameter and

brought over the flame: Though at such a distance, it will instantly extinguish the flame, if it be *cold:* but if it be held above the flame, so as to be slightly heated, the flame may be passed through it without being extinguished. That the effect depends entirely upon the power of the metal to abstract the heat of flame, is shown by bringing a glass capillary ring of *the same* diameter and size over the flame; this being a much worse conductor of heat, will not extinguish it even when *cold.* If its size however be made greater, and its circumference smaller, it will act like the metallic wire, and require to be heated to prevent it from extinguishing the flame.*

Suppose a flame divided by the wire-gauze into smaller flames, each flame must be extinguished in passing its aperture till that aperture has attained a temperature sufficient to produce the permanent combustion of the explosive mixture.

A flame of sulphur may be made much smaller than that of hydrogene, that of hydrogene smaller than that of a wick fed with oil, and that of a wick fed with oil smaller than that of carburetted hydrogene; and a ring of cool wire which instantly extinguishes the flame of carburetted hydrogene, only slightly diminishes the size of a flame of sulphur of the same dimensions.

Where rapid currents of explosive mixtures are made to act upon wire-gauze, it is of course much more rapidly heated; and therefore the same mesh which arrests the flames

* Let a small globe of metal of $\frac{1}{20}$ of an inch in diameter made by fusing the end of a wire be brought near a flame of $\frac{1}{30}$ in diameter, it will extinguish it when cold at the distance of its own diameter; let it be heated, and the distance will diminish at which it produces the extinction; and at a white heat it does not extinguish it by actual contact, though at a dull red heat it immediately produces the effect.

of explosive mixtures at rest, will suffer them to pass when in rapid motion; but by *increasing* the cooling surface by diminishing the size, or increasing the depth of the aperture, all *flames*, however rapid their motion, may be arrested. Precisely the same law applies to explosions acting in close vessels: very minute apertures when they are only few in number will permit explosions to pass, which are arrested by much larger apertures when they fill a whole surface. A small aperture was drilled at the bottom of a wire-gauze lamp in the cylindrical ring which confines the wire-gauze; this, though less than $\frac{1}{18}$ of an inch in diameter, passed the flame and fired the external atmosphere, in consequence of the whole force of the explosion of the thin stratum of the mixture included within the cylinder driving the flame through the aperture; though, had the whole ring been composed of such apertures separated by wires, it would have been perfectly safe.

Nothing can demonstrate more decidedly than these simple facts and observations, that the interruption of flame by solid tissues permeable to light and air, depends upon no recondite or mysterious cause, but to their cooling powers, simply considered as such.

When a light included in a cage of wire-gauze is introduced into an explosive atmosphere of fire-damp at rest, the maximum of heat is soon obtained, the radiating power of the wire, and the cooling effect of the atmosphere, more efficient from the mixture of inflammable air, prevents it from ever arriving at a temperature equal to that of dull redness. In rapid currents of explosive mixtures of fire-damp, which heat common gauze to a higher temperature, twilled gauze,

in which the radiating surface is considerably greater, and the circulation of air less; preserves an equal temperature. Indeed the heat communicated to the wire by combustion of the fire-damp in wire-gauze lamps, is completely in the power of the manufacturer, for by diminishing the apertures and increasing the mass of metal, or the radiating surface, it may be diminished to any extent.

I have lately had lamps made of thick twilled gauze of wires of $\frac{1}{40}$, sixteen to the warp, and thirty to the weft, which being rivetted to the screw, cannot be displaced; from its flexibility it cannot be broken, and from its strength cannot be crushed, except by a very strong blow.

Even in the common lamps the flexibility of the material has been found of great importance, and I could quote one instance of a dreadful accident having been prevented, which must have happened had any other material than wire-gauze been employed in the construction of the lamp: and how little difficulty has occurred in the practical application of the invention, is shown by the circumstance, that it has been now for ten months in the hands of hundreds of common miners in the most dangerous mines in Britain, during which time not a single accident has occurred where it has been employed, whilst in other mines, much less dangerous, where it has not yet been adopted, some lives have been lost, and many persons burned.*

* Plates of different forms of this lamp are annexed. (Pl. V.) They are applicable to all purposes in which explosions or inflammations are to be guarded against, whether from fire-damp, or carburetted hydrogene, coal gas, vapours of spirits, or of ether. And by the introduction of glass cylinders within the wire-gauze cylinder *above* the flame, the wick may be made very large, and it burns on the principle of the Liverpool lamp.

'The facts stated in Section II. explain why so much more heat is obtained from fuel when it is burnt quickly; and they show that in all cases the temperature of the acting bodies should be kept as high as possible, not only because the general increment of heat is greater, but likewise, because those combinations are prevented which at lower temperatures take place without any considerable production of heat: thus, in the Argand lamp, the Liverpool lamp, and in the best fireplaces, the increase of effect does not depend merely upon the rapid current of air, but likewise upon the heat preserved by the arrangements of the materials of the chimney, and communicated to the matters entering into inflammation.

These facts likewise explain the methods by which temperature may be increased, and the limit to certain methods. Currents of flame, as it was stated in the last section, can never raise the heat of bodies exposed to them, higher than a certain degree, their own temperature; but by compression, there can be no doubt, the heat of flames from pure supporters and combustible matter may be greatly increased, probably in the ratio of their compression. In the blow-pipe of oxygene and hydrogene, the maximum of temperature is close to the aperture from which the gases are disengaged, i. e. where their density is greatest. Probably a degree of temperature far beyond any that has been yet attained may be produced by throwing the flame from compressed oxygene and hydrogene into the voltaic arc, and thus combining the two most powerful agents for increasing temperature.

The circumstances mentioned in this Paper, combined with those noticed in the Paper on flame printed in Mr. BRANDE's

Journal of Science and the Arts, explain the nature of the light of flames and their form. When in flames pure gaseous matter is burnt, the light is extremely feeble: the density of a common flame is proportional to the quantity of solid charcoal first deposited and afterwards burnt. The form of the flame is conical, because the greatest heat is in the centre of the explosive mixture. In looking steadfastly at flame, the part where the combustible matter is volatilized is seen, and it appears dark, contrasted with the part in which it begins to burn, that is where it is so mixed with air as to become explosive. The heat diminishes towards the top of the flame, because in this part the quantity of oxygene is least. When the wick increases to a considerable size from collecting charcoal, it cools the flame by radiation, and prevents a proper quantity of air from mixing with its central part; in consequence, the charcoal thrown off from the top of the flame is only red hot, and the greater part of it escapes unconsumed.

The intensity of the light of flames in the atmosphere is increased by condensation, and diminished by rarefaction, apparently in a higher ratio than their heat, more particles capable of emitting light exist in the denser atmospheres, and yet most of these particles in becoming capable of emitting light, absorb heat; which could not be the case in the condensation of a pure supporting medium.

The facts stated in Section I. show that the luminous appearances of shooting stars and meteors cannot be owing to any inflammation of *elastic* fluids, but must depend upon the ignition of solid bodies. Dr. Halley calculated the height of a meteor at ninety miles, and the great American meteor

which threw down showers of stones, was estimated at seventeen miles high. The velocity of motion of these bodies must in all cases be immensely great, and the heat produced by the compression of the most rarefied air from the velocity of motion must be probably sufficient to ignite the mass; and all the phenomena may be explained, if *falling stars* be supposed to be small solid bodies moving round the earth in very eccentric orbits, which become ignited only when they pass with immense velocity through the upper regions of the atmosphere, and if the *meteoric bodies* which throw down stones with explosions be supposed to be similar bodies which contain either combustible or elastic matter.

Cobham-hall, Kent,
January 8, 1817.

VIII. *Some new experiments and observations on the combustion of gaseous mixtures, with an account of a method of preserving a continued light in mixtures of inflammable gases and air without flame. By Sir* Humphry Davy, *F. R. S. LL. D. V. P. R. I.*

Read January 23, 1817.

In a Paper read before the Royal Society at their last two meetings, I have described the phenomena of the slow combustion of hydrogene and olefiant gas without flame. In the same paper I have shown, that the temperature of flame is infinitely higher than that necessary for the ignition of solid bodies. It appeared to me, therefore, probable, that in certain combinations of gaseous bodies, for instance, those above referred to, when the increase of temperature was not sufficient to render the gaseous matters themselves luminous; yet still it might be adequate to ignite solid matters exposed to them. I had devised several experiments on this subject. I had intended to expose fine wires to oxygene and olefiant gas, and to oxygene and hydrogene during their slow combination under different circumstances, when I was accidentally led to the knowledge of the *fact*, and, at the same time, to the discovery of a new and curious series of phenomena.

I was making experiments on the increase of the limits of the combustibility of gaseous mixtures of coal gas and air by increase of temperature. For this purpose, I introduced a small wire-gauze safe-lamp with some fine wire of platinum

fixed above the flame, into a combustible mixture containing the maximum of coal gas, and when the inflammation had taken place in the wire-gauze cylinder, I threw in more coal gas, expecting that the heat acquired by the mixed gas in passing through the wire-gauze would prevent the excess from extinguishing the flame. The flame continued for two or three seconds after the coal gas was introduced; and when it was extinguished, that part of the wire of platinum which had been hottest remained ignited, and continued so for many minutes, and when it was removed into a dark room, it was evident that there was no flame in the cylinder.

It was immediately obvious that this was the result which I had hoped to attain by other methods, and that the oxygene and coal gas in contact with the hot wire combined without flame, and yet produced heat enough to preserve the wire ignited, and to keep up their own combustion. I proved the truth of this conclusion by making a similar mixture, heating a fine wire of platinum and introducing it into the mixture. It immediately became ignited nearly to whiteness, as if it had been itself in actual combustion, and continued glowing for a long while, and when it was extinguished, the inflammability of the mixture was found entirely destroyed.

A temperature much below ignition only was necessary for producing this curious phenomenon, and the wire was repeatedly taking out and cooled in the atmosphere till it ceased to be visibly red; and yet when admitted again, it instantly became red hot.

The same phenomena were produced with mixtures of olefiant gas and air. Carbonic oxide, prussic gas and hydrogene, and in the last case with a rapid production of water;

and the degree of heat I found could be regulated by the thickness of the wire. The wire, when of the same thickness, became more ignited in hydrogene than in mixtures of olefiant gas, and more in mixtures of olefiant gas than in those of gaseous oxide of carbon.

When the wire was very fine, about the $\frac{1}{80}$ of an inch in diameter, its heat increased in very combustible mixtures, so as to explode them. The same wire in less combustible mixtures only continued bright red, or dull red, according to the nature of the mixture.

In mixtures not explosive by flame within certain limits, these curious phenomena took place whether the air or the inflammable gas was in excess.

The same circumstance occurred with certain inflammable vapours. I have tried those of ether, alcohol, oil of turpentine and naphtha. There cannot be a better mode of illustrating the fact, than by an experiment on the vapour of ether or of alcohol, which any person may make in a minute. Let a drop of ether be thrown into a cold glass, or a drop of alcohol into a warm one. Let a few coils of wire of platinum of the $\frac{1}{60}$ or $\frac{1}{70}$ of an inch be heated at a hot poker or a candle, and let it be brought into the glass; it will in some part of the glass become glowing, almost white hot, and will continue so as long as a sufficient quantity of vapour and of air remain in the glass.

When the experiment on the slow combustion of ether is made in the dark, a pale phosphorescent light is perceived above the wire, which of course is most distinct when the wire ceases to be ignited. This appearance is connected with the formation of a peculiar acrid volatile substance possessed of acid properties.

The chemical changes in general produced by slow combustion appear worthy of investigation. A wire of platinum introduced under the usual circumstances into a mixture of prussic gas, (cyanogen) and oxygene in excess became ignited to whiteness, and the yellow vapours of nitrous acid were observed in the mixture. And in a mixture of olefiant gas non-explosive from the excess of inflammable gas, much carbonic oxide was formed.

I have tried to produce these phenomena with various metals; but I have succeded only with platinum and palladium; with copper, silver, iron, gold, and zinc, the effect is not produced. Platinum and palladium have low conducting powers, and small capacities for heat compared with other metals, and these seem to be the principal causes of their producing, continuing, and rendering sensible these slow combustions.

I have tried some earthy substances which are bad conductors of heat; but their capacities and power of radiating heat appear to interfere. A thin film of carbonaceous matter entirely destroys the igniting power of platinum, and a slight coating of sulphuret deprives palladium of this property, which must principally depend upon their increasing the power of the metals to radiate heat.

Thin laminæ of the metals, if their form admits of a free circulation of air, answer as well as fine wires; and a large surface of platinum may be made red hot in the vapour of ether, or in a combustible mixture of coal gas and air.

I need not dwell upon the connection of these facts respecting slow combustion, with the other facts I have described in the history of flame. Many theoretical views will arise from this connection, and hints for new researches, which I

hope to be able to pursue in another communication. I shall now conclude by a practical application. By hanging some coils of fine wire of platinum, or a fine sheet of platinum or palladium above the wick of his lamp, in the wire-gauze cylinder, the coal miner, there is every reason to believe, will be supplied with light in mixtures of fire-damp no longer explosive; and should his flame be extinguished by the quantity of fire-damp, the glow of the metal will continue to guide him, and by placing the lamp in different parts of the gallery, the relative brightness of the wire will show the state of the atmosphere in these parts. Nor can there be any danger with respect to respiration whenever the wire continues ignited, for even this phenomenon ceases when the foul air forms about $\frac{2}{5}$ of the volume of the atmosphere.

I introduced into a wire-gauze safe-lamp a small cage made of fine wire of platinum of the $\frac{1}{70}$ of an inch in thickness, and fixed it by means of a thick wire of platinum about two inches above the wick which was lighted. I placed the whole apparatus in a large receiver, in which, by means of a gas holder, the air could be contaminated to any extent with coal gas. As soon as there was a slight admixture of coal gas, the platinum became ignited; the ignition continued to increase till the flame of the wick was extinguished, and till the whole cylinder became filled with flame; it then diminished. When the quantity of coal gas was increased so as to extinguish the flame; at the moment of the extinction the cage of platinum became white hot, and presented a most brilliant light. By increasing the quantity of the coal gas still farther, the ignition of the platinum became less vivid. When its light was barely sensible,

small quantities of air were admitted, its heat speedily increased; and by regulating the admission of coal gas and air it again became white hot, and soon after lighted the flame in the cylinder, which as usual, by the addition of more atmospherical air, re-kindled the flame of the wick.

This experiment has been very often repeated, and always with the same results. When the wire for the support of the cage, whether of platinum, silver, or copper, was very thick, it retained sufficient heat to enable the fine platinum wire to re-kindle in a proper mixture a half a minute after its light had been entirely destroyed by an atmosphere of pure coal gas; and by increasing its thickness the period might be made still longer.

The phenomenon of the ignition of the platinum takes place feebly in a mixture consisting of two of air and one of coal gas, and brilliantly in a mixture consisting of three of air and one of coal gas: the greater the quantity of heat produced the greater may be the quantity of the coal gas, so that a large tissue of wire will burn in a more inflammable mixture than single filaments, and a wire made white hot will burn in a more inflammable mixture than one made red hot. If a mixture of three parts of air and one of fire damp be introduced into a bottle, and inflamed at its point of contact with the atmosphere, it will not explode, but will burn like a pure inflammable substance. If a fine wire of platinum coiled at its end be slowly passed through the flame, it will continue ignited in the body of the mixture, and the same gaseous matter will be found to be inflammable and to support combustion.

There is every reason to hope that the same phenomena

will occur with the cage of platinum in the fire-damp, as those which have been described in its operation on mixtures of coal gas. In trying experiments in fire-damp, the greatest care must be taken that no filament or wire of platinum protrudes on the exterior of the lamp, for this would fire externally an explosive mixture. However small the mass of platinum which kindles an explosive mixture in the safe-lamp, the result is the same as when large masses are used; the force of the explosion is directed to, and the flame arrested by, the whole of the perforated tissue.

When a large cage of wire of platinum is introduced into a very small safe-lamp, even explosive mixtures of fire-damp are burnt without flame; and by placing any cage of platinum in the bottom of the lamp round the wick, the wire is prevented from being smoked. I have sent lamps furnished with this apparatus to be tried in the coal mines of Newcastle and Whitehaven: and I anxiously wait for the accounts of their effects in atmospheres in which no other permanent light can be produced by combustion.

London, Jan. 22, 1817.

Explanation of Plate V. representing different forms of the Miners' Safe-lamp, with the apparatus for giving light in explosive mixtures.

a. Represents the single cylinder of wire-gauze; the foldings *α. α. α.* must be very well doubled and fastened by wire. If the cylinder be of twilled wire-gauze, the wire should be at least of the thickness of $\frac{1}{40}$ of an inch, and of iron or copper, and 30 in the warp and 16 or 18 in the weft.

If of plain wire-gauze, the wire should not be less than $\frac{1}{60}$ of an inch in thickness, and from 28 to 30 both warp and woof.

b. represents the second top which fits upon *a.*

c. represents a cylinder of brass, in which the wire-gauze is fastened by a screw to prevent it from being separated from the lamp by any blow. *e.* is fitted into a female screw, which receives the male screw *β.* of the lamp *f.* *f.* is the lamp furnished with its safe trimmer and safe feeder for oil.

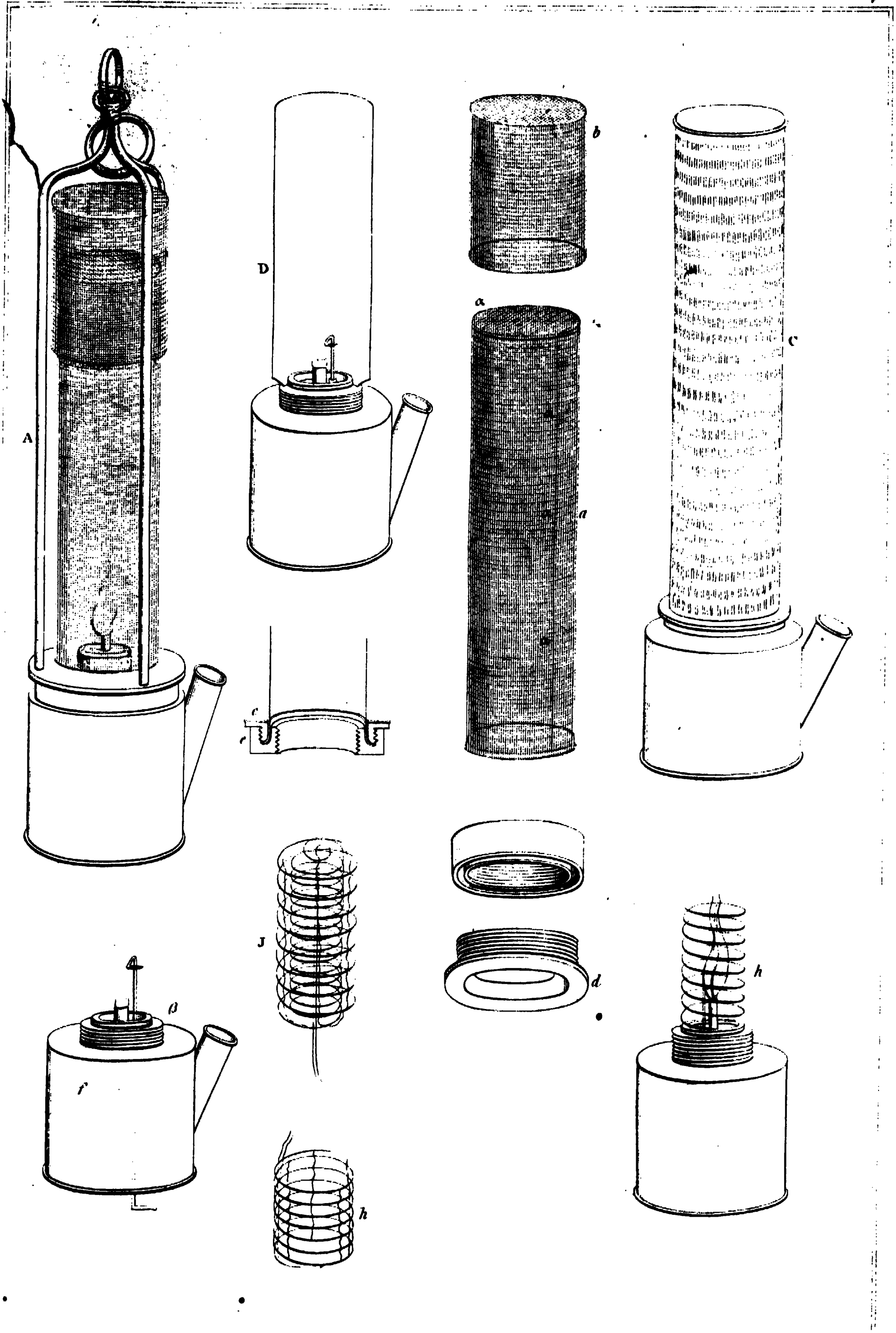
A. is the wire-gauze lamp put together with its strong wire supports, which may be three or four receiving the handle.

J. is a small cage made of wire of platinum, of $\frac{1}{70}$ or $\frac{1}{80}$ of an inch in thickness, fastened to a wire for raising it above the wick, for giving light in inflammable media, containing too little air to be explosive.

h. is a similar cage for placing in the bottom of the lamp, to prevent it from being smoked by the wick.

C. is a lamp of which the cylinder is copper of $\frac{1}{40}$ of an inch in thickness, perforated with longitudinal apertures of not more than the $\frac{1}{16}$ of an inch in length, and the $\frac{1}{30}$ in breadth. In proportion as the copper is thicker, the apertures may be increased in size. This form of a lamp may be proper where such an instrument is only to be occasionally used, but for the general purposes of the collier, wire-gauze, from its flexibility, and the ease with which new cylinders are introduced, is much superior.*

* In the first lamps which I made on this plan, more than twelve months ago, the apertures were circular; but in this case their diameters were required to be very small, as the circular aperture is the most favourable to the transmission of flame.



J. Basire

D. is a lamp fitted with a tin-plate mirror of half the circumference of the cylinder, and reaching as high as the single top, which may be used in strong currents of fire-damp to prevent the heat from rising too high.

All these forms of the wire-gauze lamp are equally safe. In the twilled-gauze lamp less fire-damp is burnt, and the radiating and cooling surface is greater, and it is therefore fitted for very explosive mixtures, or for explosive currents. The wire-gauze lamp with a double cylinder, or with a reflector, answers the same purpose.

The general principle is, that the cylinder should in no case be suffered to be heated above dull redness; and this is always effected by increasing the cooling surfaces, or by diminishing the circulation of the air.

I cannot conclude this notice respecting the safe-lamp, without stating, that in the practical application of my views I have received the most enlightened and liberal assistance from the Rev. JOHN HODGSON and Mr. BUDDLE, who have been the first persons to put my principles to the test of actual experiment in the mines, and to confide their safety to those new resources of chemistry.

IX. *De la structure des vaisseaux Anglais, considérée dans ses derniers perfectionnements. Par* Charles Dupin, *Correspondant de l'Institut de France, &c. Communicated by the Right Hon. Sir* Joseph Banks, *Bart. G. C. B. P. R. S.*

Read December 19, 1816.

Attiré dans la Grande Bretagne par le désir de rendre plus complet et moins imparfait mon ouvrage intitulé *Tableau de l'Architecture Navale aux* 18 *et* 19 *Siècles*, j'ai trouvé dans beaucoup d'officiers militaires et civils de la marine, et dans les membres de la Société Royale qui me les ont fait connaitre, cette obligeance éclairée, et, si je puis parler ainsi, cette hospitalité littéraire, qui n'appartient qu'aux cœurs bien nés et aux esprits supérieurs. Je désire que cet écrit, par lequel je voulais me rendre, auprès de mes compatriotes, l'apologiste de travaux honorables pour l'Angleterre, soit jugé par les savans et les artistes de cette contrée comme un gage anticipé de ma reconnaissance.

Un géomètre dont les découvertes, les vues, et les conseils ont fait faire les plus grands pas aux sciences physiques et mathématiques, M. de Laplace, ayant fixé son attention sur les perfectionnements que les Anglais ont introduit dans la structure de leurs vaisseaux, a senti que ces perfectionnements pouvaient avoir des conséquences importantes pour les progrès de l'ar et conduire à de nouvelles vues théoriques, qui devinssent la source de changements plus grands encore.

Il a bien voulu m'inviter à faire un examen raisonné des innovations accueillies par un gouvernement étranger, qui, généralement, voit si bien sur ses vrais intérêts. Tel est l'objet du travail dont nous rapporterons ici les seuls résultats qui puissent être d'un intérêt général pour les marines de différents peuples.

M. l'ingénieur Seppings a fait adopter un moyen de donner à la charpente des vaisseaux une force nouvelle, tant pour résister à la flexion que pour résister à la rupture. Il ne s'agit point ici d'une vague spéculation appuyée par des raisons plus ou moins spécieuses; l'expérience a prononcé de la manière la plus positive. Les vaisseaux le *Tremendous*, le *Ramillies*, et je crois l'*Albion*, ayant eu besoin d'un grand radoub nécessité par leur vétusté et par leur continuel service, ont été réparés conformément aux nouveaux principes, et remis en mer; ils ont acquis plus de rigidité, plus de solidité qu'ils n'en avaient étant neufs.

Le succès de cette première tentative a porté les Lords de l'Amirauté d'Angleterre à donner des ordres pour que l'on construisit à neuf plusieurs vaisseaux d'après le même mode de structure, et ces nouveaux essais n'ont pas été moins heureux que les premiers.

Je crois devoir rapporter ici une note que j'ai découverte dans les recherches que j'ai faites à Paris pour mon Tableau de l'Architecture Navale. Cette note porte en marge ces mots: Paris, le 5 Décembre 1811. Renvoyé par ordre de l'Empereur au Ministre de la Marine.

" Lond. 29 Nov. 1811. M. Seppings, ingénieur construc-
" teur du chantier de Chatham, a découvert un nouveau mode
" de construction pour les vaisseaux de guerre, qui promet

« plusieurs avantages importants. Son plan a été soumis « mercredi dernier, à l'Amirauté, à l'examen d'un comité « particulier composé des hommes les plus distingués par « leurs connaissances théoriques ou pratiques dans l'art de la « construction, et parmi lesquels se trouvaient Sir J. Banks, « le Dr. Wollaston, le Dr. Young, M. Rennie, ingénieur, « le Général Bentham, M. Smirke, architecte, le Capitaine « Huddart, &c. qui ont en général approuvé les principes « du nouveau mode et ont, nous n'en doutons pas, indiqué « à l'auteur les améliorations qu'ont pu leur suggérer leurs « connaissances et leur expérience scientifique. Par ce nou- « veau mode de construction on se procure une économie « très considérable de bois de chène (de 100 à 190 gros « arbres pour la construction d'un vaisseau de 74), et l'on ob- « tient plus de force et plus de durée dans leur construction. « L'essai en a été fait sur le Trémendous et a parfaitement « répondu à l'attente de l'inventeur. Non-seulement ce « vaisseau s'est montré le meilleur voilier de tous ceux qui « composaient notre escadre du nord ; mais il a éprouvé plu- « sieurs coups de vent très violents, sans en souffrir au- « cunement. Pendant toute la saison ce vaisseau a été par- « faitement sain et n'a été sujet ni aux crevasses ni aux « avaries d'aucun genre. . .

« Nous considérons le plan de M. Seppings comme étant « de la plus haute importance pour la marine, et nous ne « doutons pas qu'il ne soit suivi d'autres améliorations dans « notre architecture navale. »

Les innovations dont l'avantage est ainsi reconnu se trouvent exposées par leur auteur même dans un Mémoire inséré dans les Transactions Philosophiques de la Société Royale de

Londres, 1814; et dans un Rapport à l'Amirauté d'Angleterre, par le Dr. YOUNG, l'un des Secrétaires de cette Société. Dans ce Rapport, publié à la suite du Mémoire, le Dr. YOUNG approfondit plusieurs points importants de la théorie qui doit servir de base au système de M. SEPPINGS.

Sans égard pour les préjugés nationaux, je m'efforcerai de rendre une entière justice à toutes les innovations, à toutes les reproductions qui me paraîtront avantageuses. J'honorerai les services rendus à l'art chez un peuple étranger, comme s'ils eussent été rendus pour mon pays et par un de mes concitoyens. Mais, fidèle à cette impartialité, je revendiquerai pour les puissances maritimes autres que l'Angleterre, le droit qu'elles peuvent avoir à la priorité d'invention et de pratique dans plusieurs idées primordiales renouvellées par M. SEPPINGS.

Les anciens constructeurs français avaient si bien reconnu la vérité du principe reproduit ici par M. SEPPINGS, qu'ils l'avaient mis en usage, précisément pour parvenir au même résultat de fortifier les navires et de les empêcher de *s'arquer*. Au lieu de diriger les bordages intérieurs ou vaigres, parallèlement aux bordages extérieurs, ils avaient soin, dans toute la partie de la cale qui va depuis le faux pont jusqu'aux serres d'empature, de diriger obliquement leurs vaigres suivant les diagonales des parallélogrammes formés par les membres et les bordages; ensuite les porques couvraient les vaigres obliques, et des *pièces transversales* allaient d'une porque à l'autre suivant la direction de la seconde, diagonale de ces mêmes parallélogrammes.

Ce système, maintenu par un fort chevillage, offrait certainement une très grande rigidité. Mais il avait l'inconvé-

nient d'être plus dispendieux que le système ordinaire ; les traverses obliques situées entre les porques diminuaient la capacité de la cale, déjà fort encombrée par les porques : on croyait aussi, mais à tort, que la force longitudinale du navire était diminuée par l'obliquité des vaigres : telles sont probablement les raisons qui ont fait renoncer les François à leur ancien système.

J'ai eu entre mes mains la projection verticale de l'intérieur d'une cale, où l'on voit représentés les détails de construction que nous venons d'indiquer : le dessin original a plus d'un siècle d'antiquité ; j'en dois la connaissance et la communication à M. ROLLAND, Inspecteur adjoint du Génie maritime.

On a proposé, vers le milieu du siècle passé, de croiser le vaigrage ordinaire de nos vaisseaux par des porques *obliques* en fer : c'est ce qu'on peut voir dans l'Architecture Navale de DUHAMEL.

A l'époque où l'Académie des Sciences de Paris cherchait à diriger les efforts des savans et des artistes vers le perfectionnement de la marine, elle proposa trois fois pour sujet de ses prix, l'examen des oscillations de roulis et de tangage, et la recherche des moyens de rendre la charpente des vaisseaux plus propre à supporter les efforts résultant de ces mouvements.

CHAUCHOT, ingénieur de la marine française, remporta le prix de 1755 : et, dans un mémoire trop peu connu, renouvella l'idée de substituer des porques obliques aux porques ordinaires.

GROIGNARD, ingénieur plus célèbre, qui put encore concourir avec honneur pour le prix de 1759, sans l'obtenir, puisqu'il fut remporté par le grand EULER, GROIGNARD proposa, pour

la proue seulement, un système de bordage, de membrure, et de vaigrage qui présente des parallélogrammes fortifiés par des diagonales. Cette idée d'ailleurs ne resta point en pure spéculation, puisqu'en 1772, CLAIRON DES LAURIERS, autre ingénieur français très estimé, la mit en pratique dans la construction de la frégate l'*Oiseau*.

BOUGUER, dans son traité du Navire, et, plus tard, CHAPMAN, ingénieur Suedois, dans son *Architectura navalis mercatoria*, ont basé sur le principe reproduit par M. SEPPINGS les moyens qu'ils proposent pour donner aux vaisseaux plus de rigidité. Les ponts d'un navire, vu leur peu de courbure longitudinale, peuvent être regardés comme parallèles à la pièce intérieure placée au-dessus de la quille (la carlingue); les étançons verticaux qui supportent les ponts à l'aplomb de la carlingue forment donc avec elle et la ligne du milieu des ponts, des quadrilatères presque parallélogrammiques.

Pour empêcher ces parallélogrammes de se déformer, et par-conséquent pour empêcher le vaisseau de s'arquer, BOUGUER a placé, suivant la direction de la diagonale qui tend à s'alonger, des barres de fer fortement unies, par leurs extrémités, à la carlingue et au premier pont. Ces barres ressemblent aux *tirants* des édifices ordinaires.

CHAPMAN, au contraire, a placé suivant la direction des secondes diagonales (qui tendent à se raccourcir), des pièces de bois bien contenues sur la carlingue et sous le premier pont: ces pièces de bois, qui résistent en s'opposant à toute compression, font office *d'arc-boutants*.

Il faut conclure des développements historiques dans lesquels nous venons d'entrer, que le principe employé par M. SEPPINGS n'est nouveau ni dans la pratique, ni dans la

théorie. Mais nous n'en devons pas moins beaucoup de reconnaissance à l'homme ingénieux qui régénérant d'anciennes idées, les a dégagées de leurs inconvénients les plus graves, se les est appropriées par des modifications essentielles, et, ce qui certes n'était pas moins difficile, est parvenu à triompher de tous les obstacles qui pouvaient entrâver, empêcher même, la mise en pratique de ses utiles conceptions : essayons de les faire connaitre.

On peut reduire à quatre points principaux les innovations de M. Seppings. 1°. Remplissage de toutes les mailles au-dessous du faux pont. 2°. Suppression du vaigrage. 3° Remplacement des porques directes par des porques obliques et croisées. 4° Liaison des ponts avec le bord par des poteaux montants, une ceinture, et des courbes en fer ; obliquités opposées du bordage des ponts et des lattes qui les supportent entre les baux.

Nous allons examiner séparément chacun des trois premiers articles, qui sont tout-à-fait indépendants du quatrième. Ce dernier étant beaucoup moins important que les autres nous en supprimerons l'examen.

I. *Du remplissage des mailles.*

Remplir la membrure entre les mailles pour fortifier la charpente des navires, n'est point une idée nouvelle. Groignard l'a proposé dans son mémoire, et cet habile ingénieur a très bien fait sentir les avantages de ces moyens employés pour le *petit fond* de la carène : on peut même dire que M. Seppings n'a fait que reproduire dans son mémoire les motifs développés par Groignard à ce sujet.

Seulement Groignard se borne à remplir les mailles dans la

partie la plus basse de la carène; M. Seppings étend ce remplissage jusqu'à la hauteur du faux pont, et j'avoue que je voudrais l'étendre jusqu'au plat-bord, afin de rendre la muraille des vaisseaux moins facile à traverser par les boulets: en cela je remplirais le vœu des marins les plus habiles.

M. Seppings garnit chaque maille avec des languettes de bois frappées, les unes en dedans, les autres en dehors de la membrure. Groignard ne voulait qu'une seule pièce de remplissage ayant sur le tour l'épaisseur de la membrure, constamment introduite du dehors en dedans, un peu taillée en coin et frappée avec force pour la faire arriver à sa place. Par ce moyen il donnait à la surface inférieure de la carène une tendance à se courber en sens contraire de l'arc que le vaisseau tend à prendre lorsqu'il est à la mer.

Quelle que soit la dessication des bois employés pour la membrure et le remplissage des mailles, si l'on ne se hâte d'appliquer les bordages, il est à craindre que les bois ne se desséchent d'avantage par leur contact avec l'air. Si donc le remplissage n'avait juste que la dimension de la maille, au moindre retrait causé par la dessication, il y aurait vide entre les membres et le remplissage. On aurait ainsi perdu le principal avantage qu'on s'était proposé d'atteindre. Au contraire, si les bois sont fortement comprimés, ils pourront tendre à se resserrer sur eux-mêmes, sans cesser de se toucher, et de former une masse continue par tout également résistante.

Si nous voulons nous former une idée juste des avantages du remplissage pour conserver aux vaisseaux leur forme longitudinale primitive, observons que quand le vaisseau s'arque dans le sens de sa longueur, la partie inférieure de sa carène se raccourcit. Il y a donc un grand avantage à ne laisser

aucun vide entre les membres dans cette partie. Alors, en effet, ce raccourcissement, au lieu de s'opérer par le rétrécissement des mailles inoccupées, s'opère par la compression de pièces de bois contigues, ce qui présente une résistance beaucoup plus grande à vaincre.

En outre, le boisage du petit fond résistant par sa masse, son chevillage n'est plus fatigué par la production d'un grand arc ; la membrure et les bordages ne sont plus déchirés par les clous, les chevilles, et les gournables qui les unissent.

Si l'on remplissait les mailles au-dessus de la flottaison, ce nouveau remplissage s'opposerait au contre-arc avec la même efficacité que le remplissage des mailles inférieures s'oppose à l'arc. Mais par ce moyen on chargerait trop les hauts du vaisseau. Il faudrait peut-être se borner à remplir les mailles, selon notre usage, par le travers des diverses préceintes.

Pour concevoir l'utilité des forces qui s'opposent au contre-arc; il faut se figurer que dans une mer fortement houleuse, où le vaisseau se présente debout à la lame, lorsque cette lame soulève la proue, elle tend à produire en cette partie, elle y produit effectivement, un contre-arc ; ce contre-arc s'avance avec la lame et ne disparait que quand elle a cessé de soulever la pouppe. Ainsi le vaisseau lui-même a des ondulations analogues à celle de la mer ; mais seulement incomparablement moins grandes.

Il est évident que dans un échouage, une membrure pleine se rompra beaucoup moins facilement qu'une membrure à mailles vides.

M. Seppings ayant l'attention de calfater les coutures entre les mailles et leur remplissage, c'est une seconde barrière

qu'il oppose aux filtrations de l'eau. Parconséquent, lors même qu'un des bordages de la carène viendrait à larguer, non-seulement l'eau ne pourrait plus entrer avec cette effrayante abondance que permet la largeur des mailles actuelles; l'eau trouverait autant de difficultés à passer dans les mailles qu'elle en trouve maintenant à pénétrer entre les bordages. Concluons donc que par le moyen employé par M. Seppings, les pompes ordinaires du vaisseau suffiront pour épuiser les eaux dans beaucoup de cas où le batiment serait perdu sans ressource, si la voie d'eau tombait par le travers de quelque maille vide.

Observons encore que le bordage extérieur, portant partout sur du plein bois, est mieux assuré, plus fort, son calfatage a plus de tenue; parce que l'étoupe, avec quelque vigueur qu'on l'enfonce, trouve partout une résistance qui l'empêche de s'échapper par l'intérieur de la couture. Cet avantage est d'autant plus grand que, par le desséchement des bordages ou par le *jeu* du batiment, les coutures sont devenues plus larges.

Mais pourquoi M. Seppings arrête-t-il son remplissage à la hauteur du faux pont? c'est, à coup sûr, dans la crainte de charger son vaisseau par des poids surabondants, et de diminuer ainsi la stabilité. Cependant ces légers inconvénients sont-ils balancés par les graves dangers qu'on éviterait en poussant le remplissage jusqu'à la hauteur du premier pont?

La ligne du faux pont étant presque d'un mètre au-dessous de la flottaison, toutes les voies que l'eau pourra s'ouvrir entre les bordages, dans cette hauteur, trouveront des mailles qui leur offriront une entrée immédiate.

C'est surtout pendant la durée d'un combat que les voies

d'eau dont nous parlons, peuvent être dangereuses : arrêtons nous sur cet objet important.

Il est évident qu'un bordage dont tous les points sont soutenus par une membrure résistera plus à un choc donné, qu'un bordage soutenu par des membres isolés, et surtout si la direction du choc passe par quelque maille.

Il est évident, par exemple, qu'un boulet ayant encore assez de force pour percer un bordage vis à vis une maille, pourrait n'en avoir pas assez, si le bordage étoit soutenu derrière le point choqué. A plus forte raison, si le boulet, après voir traversé le bordage, trouve un remplissage massif deux fois à deux fois et demi plus épais que ce bordage.

Considérons un vaisseau qui combat sous le vent et qui donne fortement à la bande ; toute la ligne de son faux pont du côté de l'ennemi se trouve émergée, les boulets qui frappent depuis cette ligne jusqu'à la première batterie, trouvant peu de résistance par la viduité des mailles, cribleront à jour cette partie. Lorsqu'ensuite le vaisseau sera forcé de virer de bord, ces ouvertures s'enfonçant tout-à-coup dans l'eau, le navire coulera bas, sans qu'il soit possible de le sauver.

Ce danger n'est point imaginaire, puisque malgré notre vaigrage entre le faux pont et le premier pont, on a vu fréquemment des vaisseaux couler ainsi, en virant de bord après avoir été maltraités lorsqu'ils combattaient sous le vent. Donc *il faut continuer le remplissage des mailles jusqu'au premier pont*.

Alors les voies d'eau formées par les boulets étant des trous cylindriques percés partout en plein bois, ils se refermeront plus facilement par la réaction d'un plus grand nombre de fibres ligneuses comprimées dans un espace donné ;

et si, par cette réaction, ces trous sont bouchés dans une seule partie de leur longueur, le passage de l'eau devient intercepté.

Nous examinerons avec détail l'influence du remplissage des mailles et de toutes les autres innovations, sur la stabilité du navire et ses autres qualités. Maintenant il nous suffit de dire que même en continuant ce remplissage jusqu'au premier pont, son centre de gravité se trouve de beaucoup au-dessous du centre de gravité du vaisseau. C'est un poids additionel avantageusement placé, qui permet d'augmente la stabilité qu'ont maintenant les vaisseaux, quoiqu'en diminuant la quantité de lest dont ils ont besoin.

Dans la structure actuelle de nos batiments, nous remplissons à très peu près la membrure dans les parties extrêmes de la pouppe et de la proue. Par-conséquent nous n'ajouterons rien aux parties qu'il importe d'alléger, et par le nouveau remplissage, nous augmenterons la force de la charpente dans ses parties les plus faibles : c'est le principe de toute bonne architecture.

Considérons maintenant les mailles sous le point de vue de la durée du navire et de sa salubrité.

Les mailles ouvertes, disent leur partisans, permettent à l'air de la cale de les suivre comme des canaux, et par là de se renouveller. Ce mouvement de l'air et son contact avec les bordages, les membres, et les vaigres empêche leur échauffement et leur donne une plus grande durée.

On répond à ces objections : lorsqu'un navire a servi quelque temps, les mailles s'obstruent, l'air n'y circule plus librement ; celui qui s'en émane étant imprégné des miasmes fournis par les immondices accumulés dans ces mailles, cet air fétide ne peut qu'altérer la santé de l'équipage.

S'il est vrai que la cause la plus puissante du dépérissement des bois est leur contact alternatif avec l'air et l'eau, les mailles n'étant que rarement exemptes d'infiltrations plus ou moins abondantes, cette alternative ne doit-elle pas tendre à la destruction des bois, au moins autant que leur simple contact ?

M. Seppings cherche à prouver que des bois en contact se conservent aussi bien que des bois isolés ; cela peut être quand ils sont bien desséchés et parfaitement sains ; mais il faut avouer que dès qu'une des pièces de bois en contact contient un germe de décomposition, elle se communique rapidement à l'autre pièce.

Il faudrait pour éviter cet inconvénient, laisser la membrure du vaisseau monteé en bois tors, sécher pendant un temps suffisant ; préparer d'avance les garnitures des mailles en leur laissant un excédent d'épaisseur suffisant pour fournir au retrait du desséchement ; ne placer ces garnitures qu'au moment de border le vaisseau ; les sécher d'abord dans une étuve ayant à-peu-près 50 à 60 degrés de chaleur ; les plonger tout chauds dans le goudron, et les laisser très lentement refroidir.

Je suis persuadé qu'avec ces précautions, qu'il serait facile de rendre peu dispendieuses, et qu'il ne faudrait d'ailleurs employer que pour garnir les mailles deux mètres au-dessus et deux mètres au-dessous du faux pont, on préviendrait les dangers de la fermentation des bois produite par l'effet du contact immédiat des pièces.

Avant de terminer cette première discussion, je crois devoir citer un fait intéressant, consigné par le Dr. Young dans son Rapport, p. 335. Transact. Philosophiques de 1814.

“ Il ne semble pas qu'il y ait le plus léger fondement à

" craindre que le remplissage rende la membrure des vais-
" seaux plus facile à dépérir. Au contraire, les membres
" du *Sandwich* ont été trouvés parfaitement sains dans la
" *moitié inférieure* de leur longueur, en contact avec les coins
" qui ont été chassés entr'eux, et complétement gâtés dans
" la moitié *supérieure* qui avait été exposée, selon la méthode
" ordinaire, à l'action de l'air humide emprisonné, et de l'eau.
" Ce résultat est parfaitement d'accord avec le petit nombre
" de faits qui ont été certifiés, rélativement aux causes géné-
" rales du dépérissement des bois."

II. *Suppression du vaigrage.*

En supprimant tout-à-fait les vaigres au-dessous du faux pont, M. Seppings met à découvert la face des membres qui se trouvait en contact avec elles. Cela permet d'ailleurs de s'assurer à tout instant s'ils ne sont ni mal liés, ni brisés, ni détériorés, &c.

Les travaux de radoub deviennent en même temps beaucoup plus faciles, lorsqu'il faut toucher à la membrure. Il suffit d'enlever la garniture des mailles dans la partie qu'on veut réparer, ce qui est bien plutôt exécuté que de *dévaigrer* dans une grande étendue.

La superficie des membres mise à découvert par la suppression des vaigres est égale à la superficie enlevée au contact de l'air par le remplissage entre les mailles. Les inconvénients qui pourraient résulter du contact des bois étant en raison des surfaces en contact, ces inconvénients ne sont donc pas augmentés quant à la membrure.

Mais dès qu'on désarrime le vaisseau, l'air peut immédiatement frapper l'intérieur des couples, ce qui vaut infiniment

mieux que de circuler avec lenteur par des mailles qui sont rarement desobstruées.

Observons d'ailleurs que les clefs frappées dans les mailles au raz des vaigres d'empature, empêchent tout courant d'air de s'établir entre les membres du petit fond; et néanmoins on convient que c'est dans cette partie que la membrure se conserve le mieux.

Le poids du vaigrage étant considérable, sa suppression a sur le déplacement et la stabilité des effets importants que nous développerons, et qui tous tendent à donner au vaisseau des qualités nouvelles.

La suppression du vaigrage permet de trouver immédiatement le lieu d'une voie d'eau, dont l'existence est manifestée par l'accroissement subit dans les eaux de la cale. Actuellement, au contraire, il faut d'abord deviner dans quelle maille est la voie; puis dans quel point de la maille: enfin, il reste la difficulté assez grande de boucher une voie qu'on ne peut atteindre immédiatement.

En prolongeant le remplissage des mailles jusqu'à la hauteur du premier pont, ainsi que nous le proposons, le vaigrage entre ce pont et le faux pont n'aura plus cet inconvénient, et nous avouons qu'il nous parait nécessaire de conserver cette partie du vaigrage.

Mais au lieu de lui donner, comme le fait M. Seppings, une direction longitudinale, je lui donnerais une direction parallèle à celle des porques obliques prolongées jusques au premier pont. Je ferais descendre ces vaigres obliques à deux mètres sous la flottaison, et pour un vaisseau de 74 je leur donnerais seulement 11 centimètres d'épaisseur, ainsi qu'à la partie correspondante des porques obliques.

Ensuite, pour renforcer ce système, je considérerais les parallélogrammes que forment ces vaigres entre deux porques obliques immédiatement consécutives, et je poserais suivant la direction de la petite diagonale de ces parallélogrammes, une bande de fer ayant un décimètre de largeur sur deux centimètres d'épaisseur. Enfin, cette bande fortement unie à ses extrémités aux porques contre lesquelles elle aboutit, serait fixée par un clou sur chaque vaigre et une cheville à écrou, vers son milieu.

Sur ce vaigrage j'appliquerais la ceinture du premier pont et celle du faux pont, que j'entaillerais de deux centimètres dans ce vaigrage. Chaque vaigre oblique, dans une étendue de trois mètres seulement, serait donc engagée invariablement à chaque bout par ces ceintures, et à son milieu par une traverse en fer. Aucun boulet ne pourrait arracher ces vaigres, et la résistance qu'elles présenteraient serait incomparablement plus grande que celle des vaigres actuelles, qui ne résistent au boulet que par l'adhérence de leur clouage. En effet, un boulet ayant assez de force pour vaincre cette adhérence, s'il n'en a pas assez pour percer la vaigre qu'il frappe, la détache et l'enlève en éclats. C'est ce qui nous explique ce fait d'expérience, que les boulets qui font le plus de mal à bord sont ceux qui viennent avec une vitesse suffisante pour traverser le bordage extérieur et la membrure, et s'amortir contre les vaigres.

Il est évident que des vaigres presque droites et longues seulement de trois à quatre mètres sont des bois de troisième et quatrième espèce, beaucoup moins chers, et beaucoup plus faciles à trouver que les vaigres principales que nous appellons

vaigres de diminution. Ce surcroit de dépense occasionné par 40 ou 50 bandes de fer serait bien plus que compensé par cette économie.

III. *Remplacement des porques ordinaires par des porques obliques.*

Le remplacement des porques ordinaires par des porques obliques complétant le nouveau systême de la charpente de la carène, nous allons maintenant nous élever à des considérations plus générales, et comparer les avantages et les inconvénients de ce systeme, envisagé dans les rapports de ses diverses parties.

Afin de mettre de l'ordre dans ces recherches nous traitons séparément les questions suivantes, qui semblent renfermer toutes les raisons essentielles pour ou contre les perfectionnements dont nous proposons l'adoption.

1°. D'après le nouveau systême, le poids du navire est-il diminué ?

2°. La construction du navire est-elle moins dispendieuse ?

3°. Les capacités de la cale sont-elles augmentées, et quel usage peut-on faire de cet accroissement d'espace ?

4°. La stabilité du navire peut-elle être rendue plus grande qu'elle n'est actuellement ?

5°. Les forces latentes du vaisseau sont-elles augmentées ?

6°. La durée du vaisseau se trouve-t-elle pareillement augmentée ?

La solution des quatre premières questions ne dépendant que de calculs simples et faciles, basés sur les données de la Marine pour laquelle on opère, nous supprimons ici ces détails, parce que nous sommes forcés d'abréger cet écrit pour

le réduire à l'étendue d'un mémoire ordinaire. Nous nous contentons de dire que les résultats de ces calculs sont tous en faveur du systême de M. SEPPINGS.

Maintenant nous allons présenter la discussion théorique des deux derniers articles.

CINQUIÈME QUESTION DU § III.

Par le nouveau systême, les forces latentes du vaisseau sont-elles augmentées?

J'appelle *forces latentes* d'un vaisseau, les résistances qu'il oppose à tout changement d'état, et qui ne manifestent leur existence que par le fait même de ces changements.

Ainsi l'inertie est une des forces latentes du vaisseau.

La rigidité, cette résistance que le vaisseau oppose à toute flexion, est encore une des forces latentes, ou plutôt le résultat d'une espèce particulière de forces latentes.

La durabilité est l'expression du résultat de ces forces, en fonction du temps.

Pour bien connaître la nature et l'action des forces latentes d'un vaisseau quelconque, il faut le supposer soumis aux forces extérieures qui peuvent agir sur lui, et voir comment, par la répartition des pressions et des tensions intérieures, ces nouvelles forces se mettent en équilibre avec les forces latentes.

La première des forces extérieures est l'attraction que le globe exerce sur le vaisseau: cette attraction étant directement proportionelle à la masse de chaque partie, elle entrainerait le tout, sans mettre en action d'autre force latente que l'inertie, si le vaisseau n'était retenu par aucun obstacle.

Mais lorsqu'un navire est à flot sur une mer tranquille et qu'il n'est pas sollicité par d'autre force extérieure que sa pesanteur, il se met bientôt en équilibre, et les répulsions du fluide dirigées de bas en haut détruisent les pressions de la pesanteur exercées de haut en bas.

Si chaque élément du vaisseau reposait immédiatement sur la mer, il en déplacerait une partie dont le poids serait égal à son poids propre, et cet élément du vaisseau n'aurait à supporter que la pression infiniment petite que le fluide exercerait sur lui.

Mais comme il n'y a qu'une partie de la surface extérieure du vaisseau qui soit en contact avec le fluide, il faut que cette partie supporte de la part du fluide, une pression susceptible de contre-balancer le poids de la masse toute entière.

Donc premièrement la surface extérieure du vaisseau supporte des pressions verticales équivalentes à son propre poids.

Cette surface supporte en chaque point des pressions dirigées perpendiculairement, et proportionnelles à l'étendue des éléments de cette surface. Il faut donc regarder la carène d'un vaisseau comme une voute dont tous les éléments sont poussés normalement suivant des forces d'autant plus grandes que l'élément est plus grand, et plus éloigné de la base de sa voute, laquelle base est ici le plan de flottaison.

Actuellement le problème général qui doit nous occuper est celui-ci.

Quel effet les forces opposées de la pesanteur du vaisseau, et des pressions du fluide produisent-elles en se mettant en équilibre avec les forces latentes du vaisseau?

Considérons d'abord la pesanteur et les pressions verticales seulement. Si l'on pouvait diviser le navire en prismes verticaux infiniment petits, ayant chacun pour poids celui de la colonne d'eau qu'ils déplacent, chacun de ces prismes serait par lui-même en équilibre, il ne tendrait donc ni à s'écarter ni à s'approcher des autres prismes; les forces latentes qui s'opposent à ces mouvements ne seraient donc pas mises en jeu. Il n'y aurait dans chaque prisme en particulier que la pression verticale des éléments supérieurs sur les inférieurs.

Il n'en est pas ainsi dans nos vaisseaux; ni lorsqu'ils n'ont encore aucun chargement, ni lorsqu'ils sont armés. Pour découvrir suivant quelles lois varient les différences de pesanteur et de déplacement des éléments du vaisseau, considérons-le d'abord dans le sens longitudinal, et ensuite dans le sens transversal.

Pour cela divisons le vaisseau par tranches verticales d'une épaisseur constante infiniment petite, les plans coupants étant tous perpendiculaires au plan vertical longitudinal, si nous partons de la pouppe pour avancer graduellement vers la proue, nous verrons que les premières tranches comprenant le tableau d'arrière, la voute, une partie des bouteilles, &c. (parties qui sont toutes hors de l'eau), ces tranches ne sont soumises à aucune répulsion de la part du fluide.

Ensuite, cette répulsion commence; elle est d'abord infiniment plus petite que le poids de la tranche dont le déplacement produit cette répulsion; bientôt la répulsion de l'eau croissant par degrés rapides approche de plus en plus d'égaler le poids de la tranche qui lui correspond.

La répulsion de l'eau croissant toujours, devient égale

au poids d'une certaine tranche placée entre la pouppe et le milieu du vaisseau, au-delà de ce terme le poids de l'eau déplacée l'emporte sur le poids de la tranche. Si l'on part de l'extrémité la plus avancée de la proue pour rétrograder vers la pouppe, on trouvera de même que le poids des tranches est d'abord infiniment plus grand que le poids de l'eau déplacée, que ces deux poids diffèrent ensuite de moins en moins; qu'ils deviennent égaux pour une certaine tranche placée entre la proue et le milieu du vaisseau, et qu'en s'approchant encore plus du milieu, la répulsion de l'eau déplacée l'emporte sur le poids des tranches.

Cette inégalité de poids et de répulsion met en jeu les forces latentes du navire et produit des effets dont l'examen est de la plus haute importance.

Puisque chaque tranche est sollicitée par deux forces directement opposées; il y a d'abord tendance à la contraction dans cette tranche, et les forces qui s'opposent à cette contraction se mettent en équilibre avec cet effort. Nous reviendrons sur cette action qui s'exerce transversalement.

La résultante des deux forces opposées est égale à leur différence, et se trouve dirigée dans le sens de la plus considérable.

Si nous voulons connaître l'effet de ces résultantes, il faut partir de la pouppe, par exemple, et prendre la somme de leurs moments par rapport à l'une des sections transversales. Cette somme sera la même que celle obtenue en considérant toutes les tranches qui sont en avant de cette section. Puisque ces deux sommes représentent deux actions opposées qui se font équilibre,

Que le vaisseau soit lège, ou qu'il ait son chargement

complet, toutes les sommes des moments obtenus ainsi, présentent des actions totales exercées de haut en bas, c'est-à-dire que dans tous les points de la longueur du navire, il est sollicité à se courber en tournant vers le bas la concavité de cette courbure. Ainsi l'arc des vaisseaux règne dans toute leur longueur.

Le vaisseau n'étant pas un corps parfaitement rigide, chacun de ces moments aura son effet, et la courbure que nous venons de définir s'étendra de la pouppe à la proue.

Mais ces moments n'ayant pas une valeur constante, on doit se demander par rapport à quels plans il faut les prendre pour qu'ils soient *un maximum* ou *un minimum*; car il est évident que pour proportionner les forces latentes aux forces déformatrices, il faudra multiplier les moyens de solidité dans les premières tranches beaucoup plus que dans les secondes.

Soit x* la distance de chaque partie du vaisseau au plan ver-

* Pour faciliter la complète intelligence de ce Mémoire aux personnes qui ne seraient pas familières avec les théories analytiques, nous allons procéder à la même recherche par la seule géométrie. (Pl. VI. fig. 3.) Soit D*k* une axe horizontal mené dans le vaisseau, depuis la pouppe jusqu'à la proue. Supposons la courbe *Dbaon* telle que les verticales B*b*, A*a* .... à partir de D*k* comprennent des espaces A*a* *b*B qui représentent le poids des tranches verticales du vaisseau. Supposons de même que Aω, ƐB, prolongements de A*a*, B*b*, se terminent à la courbe ƐƐαω telle que l'aire Aω ƐB représente constamment le poids de l'eau déplacée par la tranche dont le poids propre est représenté par A*a* *b*B. Enfin soit *o*Oω la droite qui représente le plan vertical par rapport auquel il faut que les moments soient un maximum ou un minimum: estimons ces moments.

G, Γ étant les centres de gravité des aires DO*o*, EOω, le moment des poids du vaisseau (à gauche de *o*Oω) sera ... Surf. DO*o* × GG'; et le moment de l'eau déplacée (à gauche de Oω) sera ... Surf. EOω × ΓΓ' : expressions ou GG', ΓΓ' représentent les distances de *o*Oω aux centres G et Γ.

Le moment de la force qui tend à rompre le vaisseau dans le plan *o*Oω, à gauche de *o*Oω par conséquent aussi à droites, est donc

tical quelconque pris pour plan des moments. Soit dx l'épaisseur constante des tranches infiniment minces et parallèles à ce plan, soit $\varphi(x)\,dx$ le poids de ces tranches et $\psi(x)\,dx$ le poids de l'eau qu'elles déplacent; le moment total de ces deux forces sera....

$$x.\varphi(x).dx - x\psi(x).dx.$$

et par conséquent l'intégrale totale de ces moments sera

$$\int\left\{x.\varphi(x).dx - x\psi(x).dx\right\}$$

D'après les principes du calcul infinitésimal, pour que cette grandeur soit un maximum ou un minimum, il faut qu'en faisant varier infiniment peu l'origine des x, la somme des moments ne change pas pour cela, en négligeant seulement les infiniment petits d'un ordre supérieur à la quantité dont on fait avancer ou reculer l'origine des x.

Soit δx cette dernière quantité, c'est-à-dire, la variation que toutes les ordonnées horizontales éprouvent à la fois, nous aurons immédiatement

$$\delta\int\left\{x.\varphi(x).dx - x.\psi(x).dx\right\} = o$$

Surf. DOo × GG^1—Surf, EOω ΓΓ^1.

Pour que cette valeur soit un maximum ou un minimum, il faut qu'en prenant les moments par rapport au plan nNν, parallèle à oOω, et infiniment près de lui la différence soit nulle. Or cette différence est évidemment

ON (surf. DOo — surf. EOω) + $\frac{1}{2}$ ON (surf. O on N — surf. O$\omega\nu$ N).

les surfaces OonN, O$\omega\nu$N étant infiniment petites ainsi que ON, les deux derniers termes de cette valeur disparaissent devant les deux premiers. On a donc enfin pour condition du maximum ou du minimum des moments

ON (surf. DOo — surf. EOω) = o, ou surf. DOo = surf. EOω,

ce qui veut dire que le poids de la partie du vaisseau à gauche de oOω doit être égal au poids de l'eau déplacée par cette partie.

Dans le cas où ON (surf. DOo—surf. EOω) est positif, ce qui est celui de nos vaisseaux de guerre, il est évident que le poids de la tranche du vaisseau oONν étant plus grand ou plus petit que le poids de l'eau déplacée par cette tranche, le moment pris par rapport à oOω est un minimum ou un maximum

Dans cette expression, chacune des anciennes tranches ne changeant pas de poids, $\varphi(x)$ et $\psi(x)$ restent constantes, ainsi que l'épaisseur dx de ces tranches. Seulement en reculant de δx le plan par rapport auquel se prennent les moments, on ajoute la tranche dont $\varphi(\delta x)$ représente le poids et $\psi(\delta x)$ le déplacement.

On a donc enfin

$$o = \delta\int\left\{\varphi(x) - \psi(x)\right\} x\, dx = \int\left\{\tfrac{1}{2}\left[\varphi(\delta x) - \psi(dx)\right]\right.$$
$$\left. + \left[\varphi(x) - \psi(x\right]\right\} dx \,.\, \delta x.$$

Mais si l'on observe que $\varphi(x)$ et $\psi(x)$ deviennent nuls lors qu'on fait $x = o$, puisque ces expressions correspondent au poids et au déplacement d'une tranche nulle, on verra que $\varphi(\delta x) - \psi(\delta x)$ est un infiniment petit par rapport à $\varphi(x) - \psi(x)$.

Le produit $\frac{1}{2}\left[\varphi(\delta x) - \psi(\delta x)\right] dx \,.\, \delta x$ doit donc être négligé, lorsqu'on s'arrête aux infiniment petits de l'ordre le moins inférieur. Donc enfin nous avons pour condition du maximum ou du minimum des moments qui tendent à produire l'arc.

$$o = \int\left\{\varphi(x) - \psi(x)\right\} dx \,.\, \delta x,$$

ou

$$o = \delta x \int\left\{\varphi(x) - \psi(x)\right\} . dx;$$

$\int\varphi(x)\, dx$ est le poids total des tranches que nous considérons $\int\psi(x) . dx$ est le poids du déplacement total des mêmes tranches.

Donc enfin cette équation de condition nous apprend que la somme des moments qui tendent à produire l'arc, est un maximum ou un minimum, lorsque le poids de la partie du navire en avant ou en arrière du plan origine des moments est égale au poids de l'eau déplacée par cette partie du navire.

Maintenant rien n'est plus facile que de distinguer les maxima des minima ; suivant en effet que le terme négligé sera de même signe ou de signe différent que le moment total $\int [\varphi(x) - \psi(x)]\, x \cdot dx$, la somme des moments par rapport au plan déterminé sera un *minimum* ou un *maximum*.

Mais $\varphi(\delta x) . \delta x$ est le poids de la tranche ayant δx pour épaisseur, et de même $\psi(\delta x)\, \delta x$ est le poids de l'eau déplacée par cette tranche, la quantité $\frac{1}{2}.[\varphi(\delta x) - \psi(\delta x)] . \delta x . dx$ sera donc positive ou négative, suivant que le poids de la tranche infiniment mince à partir du plan origine des moments, sera plus grand ou plus petit que le poids de l'eau déplacée par cette tranche : de là nous concluons les théorèmes suivants.

I. Lorsqu'un plan vertical coupe un navire en deux parties telles que le poids de chacune égale le poids de l'eau qu'elle déplace ; le moment de ces parties par rapport à ce plan, pour produire ou flexion ou rupture est *un maximum* ou *un minimum*.

II. Il est *un maximum* lorsque la tranche infiniment mince contigue au plan des moments, a son moment propre dirigé en sens contraire du moment total.

III. Il est *un minimum* lorsque cette tranche a son moment propre agissant dans le même sens que le moment total.

Ces résultats, remarquables pour leur généralité et leur simplicité, peuvent s'appliquer immédiatement au vaisseau divisé par tranches parallèles au maitre couple, lorsqu'on connait le poids et le déplacement de ces tranches. Nous allons en donner un exemple en choisissant le vaisseau Anglais de 74 pour lequel le Dr. Young présente les données suivantes.

Equilibre sur la mer, d'un 74 Anglais ayant 176 p. de long sur 47,5 p. de large.

Longueur prise à partir de l'arrière de la flottaison.	Poids des tranches qui correspondent à ces longueurs.	Déplacement de ces tranches.	Différence des poids aux déplacements.
49	+ 699	— 627	+ 72
20	+ 297	— 405	— 108
50	+ 1216	— 1098	+ 118
20	+ 290	— 409	— 119
37	+ 498	— 461	+ 37
176	+ 3000	— 3000	= 000

Pour repartir uniformement les différences positives et négatives entre le poids de ces tranches et leur déplacement, le Dr. Young fait diverses hypothèses que nous allons rendre sensibles par le moyen d'une figure géométrique. (Pl. VI. fig. 4.) Supposons que la droite AO ait d'étendue, les 176 pieds de longueur du vaisseau, mesurés à la flottaison; que de plus on ait,

AC = 49					
CE = 20	AE = 69	Surf. ABC = + 72	$Ab = \frac{1}{3}$ AC	$= \frac{1}{3}$ 49	= 16,3
EG = 50	AG = 119	Surf. CDE = — 108	$Ad =$ AC + $\frac{1}{2}$ CE =	49 + 10	= 59
GH = 6,6	AH = 125,6	Surf. EFG = + 118	$Af =$ AE + $\frac{1}{2}$ EG =	69 + 25	= 94
HK = 13,4	AK = 139	Surf. HIK = — 119	$Ar =$ AH + $\frac{2}{3}$ HK =	125,6 + 8,9	= 134,5
KM = 17,5	AM = 156,5	Surf. IKM = — 155	$As =$ AK + $\frac{1}{3}$ KM =	139 + 5,8	= 144,8
MO = 19,5	AO = 176	Surf. MNO = + 192	$An =$ AM + $\frac{2}{3}$ MO =	156,5 + 13	= 169,5

Par ces hypothèses et ces calculs, ainsi qu'il est facile de le voir, les triangles ABC, CDE, EFG, HIK, IKM, MNO, représentent les différences positives et negatives, du poids des tranches au poids de l'eau déplacée; et les distances A*b*, A*d*, A*f*, A*r*, A*s*, A*n*, sont les distances respectives de l'origine des ordonnées horizontales aux centres de gravité de ces triangles.*

* Il est évident que ces centres de gravité sont déterminés dans l'hypothèse que ABC, HIK et KIM, MON sont rectangles; tandis que CDE, EFG sont isocèles.

Cette construction donne immédiatement pour condition d'équilibre entre les pressions et les répulsions,

$$o = 72^{T} \times 16,^{p}3 - 108^{T} \times 59^{pi} + 118^{T} \times 94^{pi} - 119^{T} \times 134^{p},5 - 155^{T} \times 144^{p},8 + 192^{T} \times 169^{p},5.$$

Par ce moyen les aires des triangles, divisés en tranches verticales de telle épaisseur qu'on voudra, représenteront la continuité des différences entre le poids des tranches et le poids de l'eau déplacée par elles.

Observons néanmoins que le triangle EFG, dont la base est de 50 pieds, ne doit pas être isocèle comme le suppose le Dr. Young. Le sommet de ce triangle étant le point où le poids l'emporte le plus sur le déplacement, ce point correspond évidemment à la position du grand mât qui se trouve dans cette tranche, et fait peser sur un seul point son gréments, ses mats supérieurs, les vergues et les voiles qui en dépendent. Or, le milieu Φ du navire est à $\frac{176}{2} = 88^{pi}$ du point A: donc $A\Phi - AE = 88 - 69 = 11$; mais le grand mât est en arrière du milieu et par conséquent plus voisin de A que Φ. Le sommet du triangle CDE se trouve donc trop en avant d'au moins 13 pieds Anglais.

Observons encore que pour rendre nulle la valeur des moments qu'il a calculés, le Dr. Young est obligé de transformer la différence + 37 ton, du poids de la proue à son déplacement, en $- 155 + 192 = 37$, hypothèse qui démontre l'inexactitude des données qui ont été fournies à ce savant.

Quoiqu'il en soit de ces données, servons nous des hypothèses du Dr. Young, pour montrer avec quelle facilité peuvent s'appliquer les théorèmes que nous avons fait connaître.

Reprenons les différences ABC = 72, CDE = − 108; EFG = 118; HIK = − 119, IKM = − 155, MNO = 192. Pour trouver les plans par rapport auxquels les moments des forces exprimées par ces valeurs sont en somme un maximum ou un minimum, il faut dans la fig. citée mener des perpendiculaires à AO, telles qu'elles interceptent, à droite par exemple, des aires positives et des aires négatives égales. Et d'abord puisque CDE = − 108 et ABC = 72, je puis mener dans CDE la droite Pp, telle que CDPp = − 72. J'aurai donc immédiatement EPp = 36, et par suite, DdE = $\frac{108}{2}$: PpE = 36 :: $d\mathrm{E}^2 = (10)^2 : \mathrm{E}p^2 = 2 . \frac{36.100}{108}$.....$\mathrm{E}p = 6.10\sqrt{\frac{1}{54}} = 8{,}15$.

Donc Ap = AE − 8,15 = 69 − 8,15 = 60,85.

Maintenant prenons les moments de ABC et de CDPp = CDE − EPp, par rapport à Pp nous aurons....

$pb = \mathrm{A}p - \mathrm{A}b = 60{,}85 - 16{,}3 = 44{,}55$.....	× 72 =	+ 3207,60	
$pd = d\mathrm{E} - p\mathrm{E} = 10 - 8{,}15 = 1{,}85$...	× − 108 =		− 199.80
$\frac{1}{3}p\mathrm{E} = 2{,}72$......	× − 36 =		− 97.80
Résultat	=	3207,60 − 297,60	

On voit qu'ici le moment definitif est 3207,60 − 297,77 = 2910 quantité positive, et qui tend à faire tomber l'extrémité de la pouppe.

Si nous observons que les tranches infiniment voisines de Pp pèsent moins que leur déplacement, nous verrons que le moment produit par ces tranches tend au contraire à relever la pouppe. Ce moment agissant en sens contraire du précédent, il faut d'abord en conclure que le moment positif 2910 est un *maximum*.

Si maintenant nous passons aux tranches comprises depuis

E jusqu'en G; puisque l'excès du poids de ces tranches sur leur déplacement égale $+118^{T} > 108-72$;

Il faut en conclure que je puis couper le triangle EFG par une perpendiculaire Qq telle que $EQq = 108-72 = 36$.

En observant que $EFf = \frac{1}{2}EFG = 59$, nous aurons immédiatement cette proportion, $EFf = 59 : EQq = 36 :: Ef^2 = (25)^2 : Eq^2 = \frac{36\times(25)^2}{59}$; d'où $Eq = \frac{6.25}{\sqrt{59}} = 19,5$; et $Aq = 88,5$.

Prenons la somme des moments par rapport à Qq, elle sera,

$$qb = Aq - Ab = 88,5 - 16\tfrac{1}{3} = 72\tfrac{1}{6} \ldots\ldots \times + \ 72 = +5196$$
$$dq = Aq - Ad = 88,5 - 59 \ = 29,5 \ldots\ldots \times - 108 = \qquad -3186$$
$$\tfrac{1}{3}Eq = \tfrac{19,5}{3} \ldots\ldots \qquad = \ 6,5 \ldots\ldots \times + \ 36 = + \ 234$$

$$qb \times 72^{T} + dq \times -108^{T} + \tfrac{1}{3}Eq \times 36^{T} = \ldots \qquad = +5430 - 3186$$

quantité dont la différence est positive et égale à 2244.

Mais ici le poids des tranches infiniment voisines de Qq l'emportant sur leur déplacement tend à courber le navire dans le même sens que ce moment: donc les moments qui agissent pour arquer longitudinalement le navire en Qq, à 88pi,5 de l'arrière, sont un *minimum*.

Passons ensuite aux tranches comprises depuis H jusqu'en M. Le déplacement de ces tranches l'emportant sur le poids, d'une quantité égale à $-119+155$, quantité plus grande que $72-108+118$; il faut en conclure qu'on peut couper le triangle HIM, par une perpendiculaire à la base telle que $ABC + EFG - GDE - HRr = o$, ce qui donne, $72 - 108 + 118 = HRr = 82$.

Nous aurons dans cette proportion,

$$HIK = 119 : HRr = 82 :: \overline{HK}^2 = \overline{13,4}^2 : \overline{Hr}^2 = \overline{13,4}^2 \times \frac{82}{119} = \overline{13,4}^2 \times 0,689$$

d'où $Hr = 12,37$.

Donc $Ar = AH + Hr = 125,6 + 12,37 = 137,97$.

Prenons maintenant la somme des moments par rapport à R*r*, elle sera

rb = *r*A — A*b* = 137,97 — 16,3	= 121,67	.....×+ 72 =	8760,24		
rd = *r*A — A*d* = 137,97 — 59	= 78,97	... × — 108 =			— 8528,76
rf = *r*A — A*f* = 137,97 — 94	= 43,97	.....× + 118 =	9188,46		
½*r*H =	= 4,12	.....× — 82 =			— 337,84

En sommes positives et négatives = 13948,70 — 8866,60

Ce qui donne en résultat définitif 5.082,10.

Ici, comme pour le plan mené par la verticale P*p*, les tranches infiniment voisines du plan par rapport auquel se prennent les moments ayant un poids inférieur à la répulsion de l'eau qu'elles déplacent, le moment de ces tranches agit en sens contraire du moment total, ainsi la somme + 5.082,10 est *un maximum* de moments. Il est évident qu'aux extrémités A et *o* la somme des moments étant nulle est un minimum. Voilà donc enfin quelle est la série des valeurs *minima* et *maxima* des moments qui tendent à faire arquer le vaisseau que nous examinons.

à zéro en A	à 60,85 = A*p*	à 88,53 = A*q*	à 137,97 = A*r*	à 176 = A*o*
Minimum	Maximum	Minimum	Maximum	Minimum
=	=	=	=	=
0	2.910	2.244	5.082,10	0

Le Dr. Young a calculé les moments de 22 pieds en 22 pieds, depuis l'arrière jusqu'à l'avant, et il a trouvé la série suivante.

à zéro	à 22 pi	à 44 pi	à 66 pi	à 88 pi	à 110 pi	à 132 pi	à 154 pi	à 176 pi
=	=	=	=	=	=	=	=	=
0	605,000	1.993,000	2.815,000	2.244,000	2.665,000	4.610,000	1.875,000	0

Si nous comparons nos résultats avec ceux-ci nous voyons d'abord que à 88 pieds, la somme des moments indiqués par le Dr. Young est plus forte que celle qui nous donne, à 88^{pi},53, le *minimum* des moments ; ce qui doit être en effet.

La valeur que nous avons trouvée pour les deux *maximums*

est pareillement plus considérable que les valeurs qui les avoisinent.

Le Dr. YOUNG en calculant la valeur d'un seul maximum, (du dernier) trouve qu'il a lieu par rapport au plan qui se trouve a $141^{pi} \frac{1}{3}$ de l'arrière de la flottaison: cette valeur est plus forte que la notre, différence qui ne peut provenir que d'une erreur de calcul.

Effectivement si nous determinons la somme des moments à $141^{pi} \frac{1}{3}$ de l'arrière de la flottaison nous trouvons pour résultat 4920,3 tandis que le Dr. YOUNG trouve 5261 tonneaux qui agissent à la distance d'un pied.

Pour nous conformer aux hypothèses de ce savant, nous avons admis qu'aux deux extrémités de la flottaison les moments fussent nuls pour faire arquer le vaisseau, cela serait vrai si les œuvres mortes du vaisseau n'avaient pas un élancement à la proue et une quête à l'étambot. Ces parties tendent à s'abaisser par leur poids et parconséquent l'arc des vaisseaux n'est pas nul aux deux extrémités de leur flottaison ; il est seulement beaucoup moindre que dans les parties intermédiaires.

J'aurais cherché à faire l'application de cette théorie au vaisseau de 74 français, si les résultats offerts par M. MISSIESSY dans son Arrimage, eussent été de nature à être soumis au calcul. Ce Général présente pour résultats définitifs du balancement des poids.

	Arrière.				Avant.			
	4e	3e	2e	1e	1e	2e	3e	4e
Excès du poids sur le déplacement	60T.1028	. . .	. . .	39T.1431	26T.401	. . .	. .	57.1188
Excès du déplacement sur le poids	. . .	31.1036	7T.239	. . .	. . .	39T.1849	60 384	

Or, il résulterait de ce tableau que la charge serait de 184^{T},48lb.

plus forte que le déplacement d'une part et plus faible de $138^{T}.1508$ de l'autre, choses impossibles à concilier.

A moins de supposer que les calculs de M. MISSIESSY diffèrent extrêmement de notre arrimage actuel, ce qui n'est pas, on doit voir que le point où les moments qui tendent à produire l'arc exercent leur maximum d'action, est dans la $9^{ème}$ tranche, avant, fort près de la seconde tranche.

Si l'on réflechit que c'est vers ce point que commence le gaillard d'avant, et que finissent les passavants, on verra que ce doit être dans cette partie que la coque du navire présente la moindre résistance à l'arc, et par conséquent le courbe d'avantage: c'est donc ce point qu'il faut fortifier par tous les moyens de l'art, soit en augmentant la solidité de la coque, soit en y accumulant une plus grande quantité de poids.

Nous arrivons donc à ce résultat bien remarquable, et qui semble paradoxal, c'est que pour uniformiser l'arc et le rendre moins grand, il est avantageux de ramener dans la position intermédiaire entre le maitre couple et l'étrave, non-seulement les poids qui sont le plus vers l'avant, mais une partie de ceux qui sont voisins du maître couple.

Examinons maintenant l'effet général des mouvements qui sollicitent les deux extrémités du navire à s'abaisser. Cette déformation ne peut s'opérer que par le raccourcissement de la quille et des parties inférieures du vaisseau, et par l'alongement des parties supérieures.

Dans chaque tranche verticale la somme des résistances produites par cet alongement et ce raccourcissement aura pour expression mathématique les moments que nous avons évalués par rapport à cette tranche. Il y faudra joindre encore une

autre action que l'on néglige ordinairement comme trop peu énergique, c'est la pression horizontale et longitudinale de l'eau. Cette pression tendant à raccourcir la quille et les parties inférieures du vaisseau, tend à rendre l'arc plus considérable : c'est ce qu'a parfaitement fait voir le Dr. Young. Puisque les parties longitudinales du vaisseau s'alongent d'autant plus par l'arc qu'elles sont plus élevées, et se raccourcissent d'autant plus qu'elles sont plus basses, il faut en conclure qu'à une certaine hauteur elles ne se raccourcissent ni ne s'alongent.

Il faudrait des calculs immenses et des expériences nombreuses pour déterminer théoriquement la position de ce point de chaque tranche verticale où les parties longitudinales restent d'une longueur constante, malgré l'arc que prend le vaisseau. Mais, sans entreprendre ce travail, il est facile d'avoir des limites suffisamment approchées de la verité.

Il me semble que le plan de flottaison est à-peu-près celui qui contient les parties invariables dans leur longueur, malgré l'effet de l'arc. Si d'une part, en effet, nous supposons que la carène entière est refoulée, tandis que l'œuvre morte est tirée pour s'étendre, ces forces se balanceront sensiblement.

Si les matériaux dont est composé le navire étaient parfaitement combinés et de plus avaient une élasticité parfaite, ils exerceraient une réaction égale à l'action ; ils reprendraient leurs dimensions naturelles aussitôt que la cause perturbatrice aurait suspendu son action.

Mais ces matériaux n'étant qu'imparfaitement élastiques, le vaisseau ne reprend qu'imparfaitement sa forme primitive : il faut donc considérer le vaisseau lui-même comme un corps

dont l'élasticité est imparfaite sans doute; mais encore très réelle et très efficace.

L'expérience est ici d'accord avec la théorie, elle fait voir qu'en changeant la distribution des poids qui chargent le navire; qu'en ajoutant d'autres poids ou qu'en supprimant quelques uns des premiers, les variations qui résultent de ces dérangements dans la valeur des moments qui font arquer le vaisseau, se manifestent par des variations très sensibles sur cet arc.

J'ai relevé l'arc d'un vaisseau lorsqu'il était encore démâté; ensuite lorsqu'on eut placé son grand mât, enfin après qu'on eut placé son mât de mizaine, puis son beaupré, et son mât d'artimon. Le poids du grand mât diminua la flèche de cet arc, mais les autres mâts, placés vers la pouppe et la proue, augmentèrent ensuite cette même flèche. Or ici l'effet des derniers mâts est dû à la flexibilité longitudinale du vaisseau, tandis que l'effet produit par le grand mat est dû à la réaction de l'élasticité.

J'ai fait des observations d'un genre analogue sur le vaisseau à trois ponts, l'*Austerlitz,* au moment de son entrée dans le bassin de Toulon. Un bâtiment de ce rang tirant trop d'eau vers son arrière pour entrer naturellement dans le bassin, on soulève la pouppe, en plaçant sous elle un ponton qu'on fait émerger à cette partie. Cette action équivaut à supprimer une partie du poids de la pouppe; et parconséquent à diminuer les moments qui tendent à produire l'arc: aussi cet arc diminue-t-il d'une manière considérable pendant cette opération.

On a cru pendant long-temps que l'arc très fort que prennent

les vaisseaux à l'instant de leur lancement provenait des efforts violents qu'ils avaient à supporter au moment de leur mise à l'eau en descendant sur une cale rapide. Cela est vrai lorsque cette cale ne se prolonge pas assez avant dans la mer, pour que le navire se mette de lui-même à flot avant de la quitter; mais dans tout autre cas la grandeur de l'arc n'est dûe qu'à l'extrême différence qui se trouve entre la distribution des poids et des déplacements.

En effet, dans l'armement des vaisseaux, les tranches dans lesquelles on conçoit leur longueur divisée, augmentent toutes de poids en même temps, mais plus vers le milieu que vers les extrémitès. A mesure que ce chargement avance, le déplacement des tranches extrêmes croit d'une quantité qui se rapproche de l'accroissement éprouvé par le déplacement des tranches du milieu. Les différences des poids au déplacement diminuent donc de plus en plus vers les extrémités; les moments diminuent pareillement.

Il faut donc poser en principe que dans le système actuel d'armement de nos vaisseaux, leur arc est un *maximum* lorsqu'ils sont lèges, et un *minimum* lorsqu'ils sont complètement armés.

C'est pour diminuer la valeur maximum qu'on a soin de lester le milieu du navire avant de le mettre a la mer, et tant que le vaisseau reste lège.

Les perfectionnements apportés depuis quelques années dans l'arrimage des vaisseaux, ont surtout eu pour but de diminuer la valeur minimum de l'arc, celle qui a lieu lorsque le batiment est armé et prêt à faire voile.

Dès qu'un navire a complетté son armement, sa charge

diminue graduellement par des consommations journalières, les moments qui tendent à reproduire l'arc varient tous les jours, et cet arc doit varier aussi.

Un des perfectionnements de l'arrimage, est d'avoir placé vers les extrêmités, la majeure partie des objets consommables et dont le poids ne peut être remplacé pendant le cours de la navigation. Par ce moyen les moments diminuent au lieu d'augmenter lorsque le vaisseau s'allégit.

Ainsi, dans le système de notre arrimage actuel, les forces qui tendent à produire l'arc sont à leur maximum au moment où le chargement est complet, en considérant ces moments comparativement avec ceux qui ont lieu pendant les consommations journalières des munitions.

Ici se présente une question importante et dont on n'a pas encore tenté de faire un examen approfondi. Quel est l'effet général de l'arc des vaisseaux sur leurs qualités a la mer? L'arc est-il avantageux ou nuisible? doit-on chercher à le diminuer, ou à l'augmenter, ou à le laisser tel qu'il résultera de la nature des matériaux employés et de la perfection ou de l'imperfection de la structure et de la construction? Essayons de repandre quelque jour sur ces questions qui nous semblent du plus haut intérêt pour le perfectionnement de l'architecture navale.

Ainsi que nous l'avons vu précédemment, lorsque le navire est en repos sa partie inférieure n'en éprouve pas moins un raccourcissement, et sa partie supérieure un alongement dont l'effet est, 1° d'alonger ou de raccourcir les fibres du bois, 2° de détruire les assemblages de la charpente, 3° de plier ou briser les clous ou les chevilles qui unissent les pièces en contact.

A mesure que les moments des forces déformatrices augmentent, ces effets augmentent pareillement : mais ils ne diminuent pas dans le même rapport lorsque ces moments diminuent ; parce que les déformations dont nous venons d'indiquer l'existence sont produits sur des corps imparfaitement élastiques.

Ainsi lorsque l'arc diminue, les clous et les chevilles se redressent, mais trop peu ; les assemblages disjoints ne se rejoignent qu'en partie : enfin les fibres alongées ne se retirent pas assez, et les fibres foulées ne reviennent pas à leur longueur primitive.

Il n'y a donc plus connexion intime entre les éléments de l'édifice, et ce défaut de connexion produit des effets d'une énergie extraordinaire sur la charpente des vaisseaux.

La non-connexion de ces éléments permet à chacun d'eux de prendre un mouvement libre plus ou moins considérable par rapport à ceux auxquels il était dans l'origine invariablement uni. L'ensemble de ces petits mouvements est ce qu'on appelle le *jeu* de la charpente.

Lorsqu'un édifice a du *jeu* dans ses diverses parties et qu'il est sollicité par des forces déformatrices quelconques, ces forces ont pour premier effet de déplacer les éléments de l'édifice suivant les directions qu'ils peuvent prendre en vertu de leur jeu : ces éléments n'opposent à ce premier déplacement que leur force d'inertie ; et la quantité des forces vives dont le systéme est animé n'est en rien diminuée.

Mais chaque élément, en éprouvant de la sorte un déplacement libre, acquiert une certaine vitesse au moment où il éprouve la résistance des autres parties du systême ; il y produit un choc.

Ainsi ce n'est plus par une simple pression que les éléments de l'édifice agissent les uns sur les autres pour s'alonger ou se raccourcir mutuellement; et comme le choc augmente prodigieusement l'énergie de la force perturbatrice, on voit que, toutes choses égales d'ailleurs, et les forces déformatrices restant les mêmes, le jeu des pièces doit sans cesse augmenter pour produire des effets de plus en plus dangereux.

Ce choc est donné par une vitesse pour ainsi dire insensible lorsqu'il résulte de variations lentes exercées dans le chargement du vaisseau; mais il est rapide et violent dans les perturbations produites par les forces de la nature.

Il ne faut pas appliquer à la structure d'un vaisseau les idées qu'on pourrait se former sur la structure d'un édifice établi sur un sol immuable, et sans qu'aucune force déformatrice vienne ajouter son action à celle de la pesanteur des éléments de cet édifice. Il faut considérer le vaisseau lorsqu'il flotte sur une mer plus ou moins agitée, lorsqu'il est battu par des vents plus ou moins forts, plus ou moins constants, plus ou moins brusques.

Alors on verra que les moments qui tendent à produire l'arc du vaisseau varieront, pour ainsi dire, à chaque instant, qu'ils deviendront même vers la pouppe et vers la proue alternativement positifs et négatifs. Il faut donc regarder un vaisseau battu par la mer et les vents, comme une espèce de reptile qui nage à la superficie d'une mer ondulée, qui se courbe et se recourbe à chaque instant dans le plan vertical de sa route, et s'avance en formant de la sorte une ligne sinueuse.

Lors même qu'on regarderait l'élasticité des bois comme une force que le temps ne peut point altérer, ce qui n'est pas, il

est facile de voir qu'en divisant la durée des vaisseaux en intervalles égaux, le jeu de leur charpente, et parconséquent l'arc qui en résulte, doit croitre suivant une marche accélérée. Ainsi, toutes choses égales d'ailleurs, l'arc des vaisseaux augmente plus a leur seconde campagne qu'à leur première; à leur troisième qu'à leur seconde, et ainsi de suite; c'est aussi ce que confirme l'expérience. Une première campagne n'augmentera pas l'arc d'un bon vaisseau de plus de 3 ou 4 centimètres; il s'accroitra de 10 ou de 15 à la quatrième ou cinquième, et souvent cette seule augmentation du jeu de la charpente d'un vaisseau, nécessitera d'en faire un grand radoub.

D'après ces détails on doit voir que la durée des vaisseaux, toutes choses égales d'ailleurs, est directement proportionnelle à leur inflexibilité virtuelle ou primitive. Or cette inflexibilité est en raison inverse de la flèche de l'arc longitudinal. La durée des vaisseaux considerée sous ce point de vue est, comme on voit, en raison inverse de l'arc qu'ils prennent au moment de leur mise à l'eau; leur construction étant totalement finie, ou du moins également avancée.

Aussi les ingénieurs regardent-ils comme un indice de la faiblesse de leurs constructions, la grandeur de l'arc au moment de sa mise à l'eau; j'en ai vu plusieurs cacher ce véritable arc et le faire croire beaucoup plus petit qu'il n'était réellement. Mais un semblable charlatanisme est indigne d'un corps aussi éclairé que celui du génie maritime; et des erreurs de fait, ainsi présentées d'une manière positive, pourraient être les germes de conséquences pareillement erronnées et tout-à-fait contraires aux progrès de l'architecture navale.

Tout en convenant que la flexibilité virtuelle des vaisseaux

est contraire à leur durée, beaucoup de marins ont pensé qu'elle leur procuroit des qualités nautiques et spécialement une plus grande vélocité. C'est dans cette persuasion qu'on a vu des batiments chassés par un ennemi supérieur employer tous les moyens possibles pour délier leur navire afin de lui procurer une marche plus avantageuse. Mais comme ils employaient ce moyen, en même temps qu'ils jettaient à la mer tous les poids les plus élevés dont ils pouvaient se débarasser, et qu'ainsi la stabilité se trouvait augmentée plutôt que diminuée, malgré l'émergement; ils pouvaient, dans un gros temps, conserver la même voilure et même l'augmenter pour forcer de voiles. C'est par la réunion de toutes ces causes que le vaisseau pouvait acquérir des qualités nouvelles et prendre une plus grande vitesse sans qu'on fut en droit de conclure que cet accroissement de vélocité fût dû à la déliaison du vaisseau.

D'autres faits cependant semblent venir à l'appui de cette conclusion. On a vu des vaisseaux dont la marche était très médiocre dans leurs premières campagnes, en acquérir une supérieure lorsqu'ils devenaient considérablement arquées.

Observons relativement à ces faits, qu'à chaque nouvelle campagne un nouveau capitaine faisant un arrimage plus ou moins différent, et cherchant à varier la différence des tirants d'eau, ces causes diverses ont pu donner à d'anciens vaisseaux des qualités inespérées.

Je dirai de plus que si l'on cite quelques bâtiments dont la marche est devenue moins désavantageuse à mesure qu'ils ont vieilli, le plus grand nombre des navires perd au contraire, avec le temps, une partie sensible de sa vélocité.

Il me semble possible d'expliquer ces contradictions, au lieu

d'en nier la réalité, comme a fait Bouguer dans son traité du Navire.

Les anciens vaisseaux sur lesquels on a observé cette augmentation insensible de vélocité, ayant leurs plans conçus d'après les principes alors adoptés, avaient une proue beaucoup trop fine; cette proue devait donc s'enfoncer beaucoup plus dans la lame et s'émerger d'avantage aussitôt que cette lame était passée; ainsi les tangages étaient plus étendus à chaque rechute de la proue; son grand enfoncement dans le fluide lui faisait éprouver une plus grande rétardation dans sa vitesse, et le vaisseau ne pouvait pas avoir une marche excellente.

Mais par l'effet de l'arc le déplacement diminue vers le maître couple tandis qu'il augmente vers les extrêmités. La proue, d'abord trop exigue, augmentait donc peu à peu ses capacités; les défauts du batiment, qui tenaient à cette exiguité devaient progressivement diminuer et la vélocité s'accroitre.

Au contraire, lorsque la proue a déjà tout le volume qui convient à la navigation la plus avantageuse, l'effet de l'arc étant d'augmenter encore ce volume, le rend trop considérable, et la marche doit diminuer.

De là résulte donc cette conséquence singulière et remarquable. *C'est que l'arc ne peut être favorable qu'à de mauvais vaisseaux et qu'il est contraire aux bons bâtiments.* Aussi maintenant que l'architecture navale a fait des progrès sensibles on ne peut plus citer de ces bâtiments dont la marche devient supérieure avec le temps.

Il faut bien se garder de croire que l'augmentation du volume des extrêmités de la carène par l'effet de l'arc soit

une quantité toujours peu considérable. Pour un arc d'un demi-mètre, par exemple, (et l'on a vu des vaisseaux naviguer avec un arc plus considérable encore) le volume de la proue augmente *de plus de* 100 *tonnaux;* il en est de même du volume de la pouppe et le volume de la carène vers le maître coup le diminue d'autant. On voit qu'une pareille augmentation équivaut à un renflement considérable de ces extrémités.

Si l'on réfléchit encore que les tranches qui s'élèvent par l'effet de l'arc, sont celles du milieu, s'élèvent beaucoup moins que ne s'abaissent les extrêmités qui sont le plus chargées sur les hauts, à cause de la tonture générale des ponts, on verra que l'effet de l'arc est d'accroitre la stabilité : c'était donc encore un grand avantage pour les anciens bâtiments, qui généralement avaient trop peu de stabilité.

Ces batiments ayant alors une excessive tonture pouvaient en perdre beaucoup par l'effet de l'arc avant que leur batterie fût noyée vers l'avant ou vers l'arrière. Il y avait donc un grand avantage à ce que l'arc diminuât cette tonture, et fît émerger la batterie vers le maître couple ou sa hauteur au-dessus de la flottaison était trop peu considérable.

Si maintenant nous considérons le vaisseau par rapport à ses lignes d'eau, nous verrons que ces lignes assez peu changées par l'arc vers la flottaison, le seront beaucoup plus vers les parties inférieures de la carène. A tel point que si l'arc était plus grand que le $\frac{1}{4}$ de la différence de tirant d'eau, ce qui n'est pas râre, la ligne d'eau tangente au-dessus de la quille aurait la forme d'un ∞ alongé; elle serait plus large à l'avant et à l'arrière qu'au milieu, ce qui certes ne peut être favorable à la marche du batiment.

Plus les lignes d'eau sont parfaites, plus cette marche sera troublée par cette déformation, plus par conséquent l'arc aura des effets pernicieux. C'est la conséquence à laquelle nous étions déjà parvenus par des considérations tirées des effets du tangage.

Après avoir déterminé l'influence directe de l'arc sur la marche progressive des vaisseaux il faut considérer son influence sur leur marche latérale ou leur *dérive.*

Si nous concevons le vaisseau coupé par tranches parallèles au maître couple, les tranches du centre étant émergées par l'effet de l'eau, leur résistance à la dérive diminue. Au contraire les tranches de la pouppe et de la proue s'immergeant, leur résistance à la dérive est augmentée. Ainsi la résistance à la dérive, au lieu d'être augmentée le plus possible vers le centre, est au contraire accumulée vers les extrêmités par l'effet de l'arc.

Donc l'arc des vaisseaux a pour effet immédiat de rendre leurs évolutions plus difficiles et plus lentes.

Sans doute l'arc augmentant le tirant d'eau de l'avant et de l'arrière aux dépens du tirant d'eau du milieu, la partie du gouvernail plongée dans le fluide augmente en surface et peut en partie balancer cet effet ; mais il en résulte que pour effectuer la même évolution, il faut faire d'autant plus de force à la roue du gouvernail que l'arc devient plus considérable : ainsi le gouvernail et les timoniers seront d'autant plus fatigués que le vaisseau sera plus arqué.

Il faut observer ensuite que la partie émergée par l'effet de l'arc auprès du maître couple étant sensiblement verticale et parallèle à la quille avait la plus grande efficacité possible pour résister à la dérive. Au lieu que les parties immergées vers la pouppe et vers la proue, formant un angle de plus en

plus aigu avec la direction latérale s'opposent beaucoup moins à la dérive.

Concluons donc qu'un second effet de l'arc est de diminuer la résistance totale à la dérive.

Si nous résumons les résultats auxquels nous venons de parvenir nous verrons que,

1°. Tous les vaisseaux ont de l'arc.

2°. Cet arc est à son maximum dans le vaisseau lège, comparativement au vaisseau complètement armé.

3°. Cet arc du vaisseau complètement armé est à son maximum par rapport à l'arc qui résulte des consommations journalières.

4°. La forme de l'arc varie ainsi que son amplitude suivant la distribution des poids qui composent l'armement.

5°. Le point de la plus grande courbure, ou le sommet de l'arc, correspond entre les deux gaillards par le travers des passe-avants, beaucoup plus près de la proue qu'on ne le suppose ordinairement.

6°. La durée des vaisseaux, toutes choses égales d'ailleurs, est en raison inverse de leur arc primitif.

7°. L'arc augmente beaucoup les capacités de la carène vers la pouppe et la proue aux dépens des capacités du milieu.

8°. L'arc auroit un peu la stabilité.

9°. L'arc ne peut être favorable qu'à de mauvais vaisseaux.

10°. Dans les autres navires il diminue la marche directe.

11°. Dans tous il diminue la résistance latérale qui s'oppose à la dérive.

12°. Dans tous il augmente la difficulté d'évoluer, surtout avec les voiles.

13°. L'arc augmentant les capacités de la pouppe et de la

proue, aux dépens des capacités du milieu, diminue l'énergie des forces qui tendent à augmenter de plus en plus cet arc. Mais la diminution d'élasticité dont cet arc est le signe visible, fait que le navire n'en est pas plus, pour cela, susceptible de résister à des efforts moins énergiques, et c'est ce que démontre l'expérience, puisque l'accroissement de l'arc, dans une suite de temps égaux et dans les mêmes circonstances, cet accroissement, dis-je, procède par degrés de plus en plus grands, jusqu'à un certain terme où l'on ne pourrait plus se servir du vaisseau tant il serait cassé.

D'après les principes que nous venons d'établir, il est évident que les vaisseaux modernes ne peuvent que perdre de leurs qualités en s'arquant, et que tout moyen qui pourra tendre à diminuer cet arc sera pour ces vaisseaux un véritable perfectionnement, ou si l'on veut, un préservatif de dégénération. Voyons donc si le systême de Mr. Seppings est plus propre que le systême actuel à préserver les vaisseaux de s'arquer trop considérablement.

Dès le commencement de ce Mémoire nous avons rapporté l'expérience fondamentale sur laquelle cet ingénieur a basé tout son systême. J'avoue que cette expérience n'est pas présentée d'une manière assez concluante ; parce que Mr. Seppings, au lieu de considérer un charpente massive et continue comme celle des vaisseaux, compare les forces de deux assemblages à claires voies, l'un formé par des pièces parallèles qui forment des parallélogrammes, l'autre par des pièces obliques qui forment une suite de triangles.

Pour obtenir de l'expérience une conviction pleine et entière, il faudrait faire l'épreuve suivante, aussi simple que peu dispendieuse.

On ferait, sur une échelle de 10 ou 9 pour cent, deux murailles droites ayant un échantillon proportionnel à celui des vaisseaux. Si l'on adoptait l'échelle de dix pour cent, en donnant seulement quatre mètres de long et un mètre de haut à cette muraille, qu'on percerait vers le haut de deux rangées de sabords; on ferait l'une suivant le systême de membrure, de vaigrage, et de bordage ordinaires; l'autre serait faite suivant le systême de Mr. Seppings avec des porques obliques au-dessous des batteries et des traverses obliques entre les sabords.

Pour mesurer ensuite avec une grande exactitude l'arc pris par ces deux murailles tenues verticalement et chargées du même poids, on fixerait aux quatre angles de la muraille des prolonges de trois mètres seulement, ce qui donnerait une longueur totale de dix mètres et produirait des flèches beaucoup plus grandes que si l'on n'eût pas alongé les deux murailles.

Dans le mémoire approuvé par l'Institut, où j'ai présenté mes expériences sur la flexibilité des bois,* j'ai démontré que les systèmes de charpente composés de pièces d'un échantillon proportionnel, et chargés de poids proportionnels à leur propre poids, prenaient des arcs *dont le rayon de courbure était constamment le même;* parconséquent en chargeant nos deux murailles 10 fois plus que la réaction de l'eau ne tend à courber le vaisseau, proportionnellement le rayon de courbure de l'arc sera rendu 10 fois moins considérable; mais en doublant la longueur proportionnelle de la muraille, l'arc est rendu huit fois plus courbé, et en accumulant la force au milieu l'arc est accru dans un rapport approchant de deux

* Voyez Journal de l'Ecole Polytechnique.

à trois. Donc les courbures qu'on observera sur les murailles modèles seront aux courbures modèles comme $\frac{10 \times 8 \times 3}{2} : 1$ ou comme $120 : 1$.

On sait que les flèches des arcs très peu courbés sont entre eux comme les quarrés des cordes de ces arcs ; donc la corde de l'arc du modèle étant le dixième de la corde du vaisseau même, les flèches des arcs pris par les modèles seront pour une même courbure cent fois plus petites que celle du vaisseau, mais le modèle éprouve une courbure 120 fois plus considérable que le vaisseau ; donc les flèches des modèles seront les $\frac{12}{10}$ de celles des vaisseaux que ces modèles représenteront.

Pour faire cette expérience d'une manière plus concluante, il vaudrait mieux, selon moi, donner aux murailles modèles une longueur simplement égale au dixième de la longueur du vaisseau, et charger successivement cette muraille de poids, 1, 2, 3...10 fois plus forts que la puissance qui tend à produire l'arc.

Alors la flèche de l'arc de chaque muraille modèle serait 0,01 0,02, 0,03.... 0,1 de l'arc du vaisseau multiplié par $\frac{3}{2}$.

On aurait par ce moyen des flèches assez grandes pour être mesurées et comparées avec une grande précision.

Si nous cherchons à produire ici des flexions jusqu'à 15 fois aussi grandes que celles du vaisseau, c'est que plus les flexions sont grandes, plus il est facile de découvrir les anomalies qu'elles présentent dans leur accroissement progressif.

Au reste la pratique des arts nous présente un assez grand nombre d'expériences irrécusables pour n'avoir pas besoin de recourir à de nouveaux essais avant de se former une opinion

sur les résultats de celles dont nous donnons l'idée. Les portes d'écluse, par exemple, sont formées dans un système parfaitement comparable à celui renouvellé par M. Seppings; des madriers jointifs forment un premier plan; un encadrement à pièces parallèles représente la membrure, et les traverses diagonales entre ces pièces parallèles représentent les traverses et les porques obliques.

Si les ingénieurs des ponts et chaussées trouvaient quelque imperfection, quelque défaut de solidité dans ce système, ils ne manqueraient pas de lui substituer un système analogue à celui de notre membrure bordée et vaigrée; mais c'est au contraire, parce qu'ils connaissent l'infériorité de ce dernier système qu'ils se gardent bien de l'adopter linea.

Il est essentiel d'observer que les porques obliques et leurs traverses ne permettent pas plus au vaisseau de s'alonger que de se raccourcir: puisque des pièces transversales sont dirigées suivant les deux diagonales des parallélogrammes dont la cale à sa surface tapissée.

Remarquons que le vaigrage supprimé ne servait, pour ainsi dire, en rien pour empêcher l'arc du vaisseau, parce que la force de redressement tend elle-même à produire cet arc. Le vaigrâge du petit fond ne s'oppose donc qu'au raccourcissement de la quille, et le remplissage des mailles, ainsi que nous le dirons dans un moment, produit bien plus énergiquement cet effet.

Je ne parle pas des porques ordinaires, parce qu'étant perpendiculaires à la direction de l'arc, elles ne servent en rien contre cet arc.

Les vaigres depuis le faux pont jusqu'au premier pont, telles que les conserve M. Seppings, étant vers la partie du

vaisseau qui ne s'alonge ni ne se raccourcit, n'ont, pour ainsi dire, qu'une influence *minimum* contre la production de l'arc. Mais en les dirigeant obliquement, comme je le propose, le vaisseau ne pourra pas subir de flexion sans que toutes ces vaigres ne soient refoulées dans le sens de leurs fibres, ce qui les rendra capables d'une résistance incomparablement plus grande.

Enfin, le remplissage des mailles du petit fond s'opposera bien plus efficacement à ce raccourcissement progressif de la quille, que les vaigres de cette partie. Puisque la manière dont ce remplissage est chassé de dehors en dedans des mailles lui donne une force latente qui tend à faire alonger la quille et redresser le vaisseau.

Si l'on a soin de border aussitôt après avoir ainsi chassé ce remplissage, l'air extérieur ne le desséchant pas, il ne se rétrécira pas et ne donnera pas à la charpente un jeu pernicieux.

Il y a plus, lorsque le vaisseau sera mis à la mer, les eaux répandues dans l'intérieur de la cale, celles qui par l'effet de la capillarité traverseront le bordage et par suite la membrure, gonfleront cette membrure, et ses pièces étant contigues elles tendront à occuper plus d'espace et à détruire les forces de compression qui pourraient arquer le vaisseau.

Nous pourrons donc conclure qu'en adoptant le systême de M. SEPPINGS, avec la modification que je propose au vaigrage entre le premier pont et le faux pont,

1°. L'arc primitif du vaisseau sera moindre.

2°. L'accroissement progressif de cet arc sera moindre aussi.

3°. Le jeu qui doit s'établir entre les diverses parties de la charpente sera pareillement beaucoup moins considérable.

Figures de l'Examen théorique du nouveau Système de Charpente

Fig. 1.

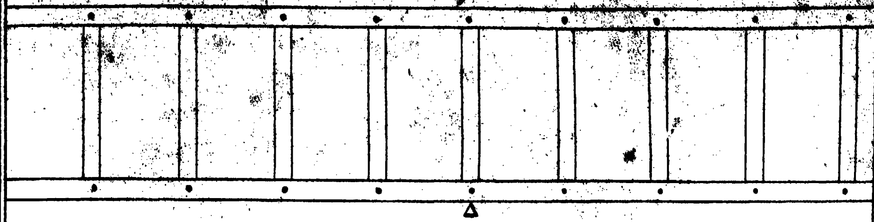
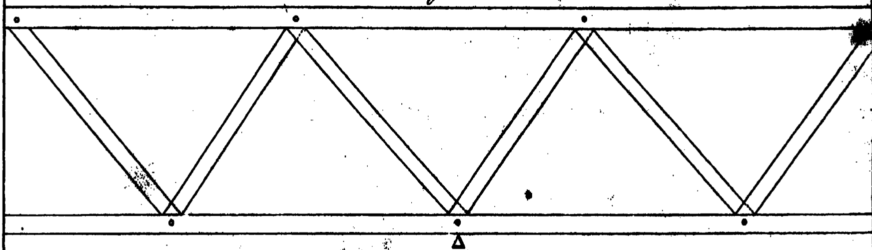


Fig. 2.



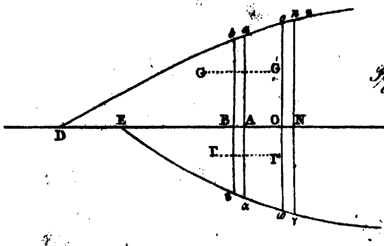


Fig. 3.

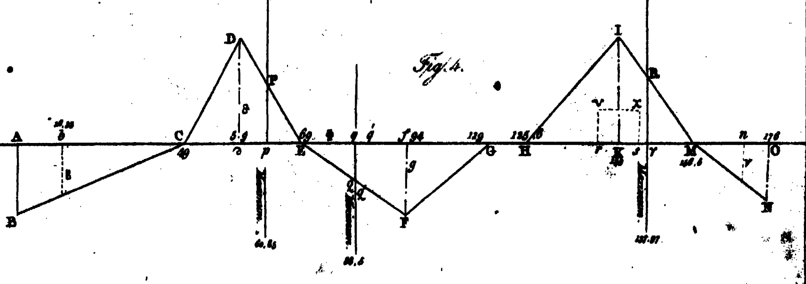


Fig. 4.

Ainsi le nouveau systême réunit à un très haut dégré la *solidité* et *la durée*.

Si l'on réfléchit maintenant que par les moyens dont nous avons démontré la possibilité, mathématiquement, en traitant de la stabilité, le lest se trouve rapproché du centre, ainsi qu'une partie des munitions : on verra que non seulement les vaisseaux acquièrent plus de force pour résister à l'arc, mais encore que la cause efficiente de cet arc devient moins considérable.

Les vaigres par leur direction longitudinale et leur grande longueur offrent au premier coup d'œil une idée de solidité plus grande que celles de pièces enchassées les unes dans les autres et laissant de grands triangles vides entre elles. Mais on sent bientôt le défaut d'une telle objection, lorsqu'on réfléchit que les vaigres de la cale, au lieu de résister contre des tensions n'ont à s'opposer en général qu'à des contractions ; ce qui rend l'effet de leur longueur à peu près nul.

X. *On a new Fulminating Platinum.* *By* Edmund Davy, *Esq. Professor of Chemistry, and Secretary to the Cork Institution. Communicated by Sir H.* Davy, *LL. D. F. R. S. V. P. R. I.*

Read February 13, 1817.

I. *Introduction.*

THE metals, as is well known, closely resemble each other in their physical and chemical characters. A chain of analogies connects them together into one class, and serves to distinguish them from all other bodies with which we are acquainted. Hence, the observation of a new property, or the discovery of a new relation in any one of the metals, is a sufficient ground for extending similar enquiries to all the others. And though the same methods may not furnish equally successful results in the case of different metals, yet by varying the processes, some new truths are usually brought to light.

The analogies that exist between the different metals, are in some cases nearer, and in others more remote; but in all instances they are sufficiently numerous and striking to serve the ends of classification, and facilitate the progress of scientific discovery. Gold, silver, and platinum, were formerly distinguished by the epithet *noble* or *perfect* metals; this distinction, though it no longer exists, was founded on a similarity in their physical properties. Gold and silver furnish with the volatile alkali, well known fulminating

compounds. Gold and platinum appear to be more closely related to each other than they are to silver, or to any of the other metals; though separated by a number of marked distinctions, they yet possess in common, many points of resemblance. They are both soluble in the same menstrua, and can only with difficulty be made to unite with oxygene, chlorine or sulphur; and their oxides form peculiar triple compounds with acid and alkaline or earthy substances. From these analogies, and especially the last, it might be presumed that platinum, like gold, by particular treatment, was capable of furnishing a fulminating compound. Since platinum has been known to chemists, different attempts have been made to produce such a compound, but without effect. On the discovery of fulminating mercury by Mr. HOWARD, he endeavoured to communicate fulminating properties to compounds of platinum, by means of alcohol, but his trials were unsuccessful. I have to a certain extent succeeded in this way, and my attempts have led to the observation of some new facts. I have also obtained a new compound of platinum, analogous in its properties and composition to *aurum fulminans*, and which, in consequence, I shall venture to designate by the term *fulminating platinum.* I have, indeed, already noticed a peculiar compound of platinum under this name,* but the term should be restricted to the new compound, on account of its superior fulminating properties. This fulminating platinum serves to extend the existing analogies between the noble metals, and fills up a vacant space in their chemical history. In the present communication, I shall endeavour to describe this substance; but before I enter

* Philosophical Magazine, Vol. 40.

on the detail of its physical and chemical properties, it may be proper to notice the methods by which it may be procured.

II. *Modes of obtaining Fulminating Platinum.*

In the year 1812, whilst I was engaged in experiments on some compounds of platinum, I obtained an ammonia sulphate of this metal, by treating sulphate of platinum with pure ammonia. On boiling a little of the ammonia sulphate with pure potash, it became of a darker colour. I supposed it might have been converted into an oxide, but I did not examine it. On recently renewing this enquiry, I was led to the discovery of fulminating platinum.

The platinum I employed, in preparing the fulminating compound, it may be proper to remark, was in the form of thin sheets, but previous to its being used it was dissolved in nitro-muriatic acid, then precipitated by muriate of ammonia, and again reduced to the metallic state.

The platinum so prepared was dissolved in nitro-muriatic acid, the solution evaporated to dryness, and the dry mass dissolved in water. A current of sulphuretted hydrogene gas was then passed through the aqueous solution, till the supernatant fluid remained colourless. The hydro-sulphuret of platinum, after being well washed and partially dried, was converted into sulphate by the agency of nitrous acid. The fulminating platinum was prepared, by treating the aqueous solution of sulphate of platinum with a slight excess of pure ammonia; the precipitate thus obtained was placed on a filter, and partially dried, so as to be easily separated from the paper. It was then put into a Florence flask, with a strong solution of pure potash, and the fluid boiled nearly to dry-

ness. A quantity of water was then added, the whole thrown on a filter, and the solid matter, after being well washed* and dried for several days at the temperature of about 212° Fahrenheit, exhibited properties, which will presently be described.

In preparing fulminating platinum, it may be here remarked, other alkaline substances as soda, common kali, &c. may be substituted for pure potash. And farther, it may be added, nearly similar results are obtained, whether the ammonia sulphate of platinum be put into the fixed alkali in fine powder, or in a moist or wet state; whether the fixed alkali be immediately added, after the precipitation from the sulphate of platinum by ammonia, or the precipitate be first separated and partially dried, previous to such treatment. In making fulminating platinum, (as is the case with aurum fulminans,) circumstances may be varied to a considerable extent, without materially impairing its properties.

III. *Properties of Fulminating Platinum.*

Fulminating platinum, when prepared in the manner I have described, appears in the form of a loosely coherent brown powder. I have obtained it of different shades of colour, from a light brown to a dark chocolate, and even almost black. These varieties of colour seem to be connected with the agencies of the fixed alkalies, and the peculiar circumstances which accompany its formation. The fulminating powder seems to be of a lighter or darker colour, as the

* The washings contain a little fulminating platinum in solution, for when neutralized by nitrous or oxalic acid, a precipitate is obtained, which when washed and dried, exhibits fulminating properties.

quantity of fixed alkali employed is smaller or greater, and the process of boiling continued for a shorter or longer time. If, after the addition of the fixed alkali the fluid be boiled down to dryness, the fulminating powder will be of a dark colour, and if the heat be still continued, it will be partially decomposed, and this effect will be accompanied with slight explosions. The differences of colour observed in the fulminating powder are not perhaps connected with any difference in its constitution; at least this variety of circumstance does not appear to deteriorate its fulminating properties in any sensible degree, except in cases where the process has been carried too far in the use of the fixed alkali, aided by long continued heat.

When the fulminating powder in small quantity is placed on bibulous or filtering paper, and gradually heated over a clear fire, or an Argand lamp, it explodes with a loud report, the paper is lacerated, and its parts violently rent asunder. A bit of the powder no bigger than the head of a pin, or about $\frac{1}{20}$ of a grain, produces a sharp crack, and makes a hole in the paper.

One grain of the powder was placed on a slip of thin sheet copper, and exploded by the heat of a taper: it produced a report louder than the discharge of a pistol, and the copper was deeply indented, as if it had received the impression of a large punch.

A $\frac{1}{4}$ of a grain of the powder was exploded on a slip of tinned iron: the lustre of its surface was tarnished, and it had the appearance of an alloy of platinum and tin. The same quantity of the powder was placed between two slips of filtering paper and exploded; both slips were perforated and

lacerated, but the greatest effect was produced on the lowest slip. The results were analogous when the powder was placed between two slips of thin sheet copper, or platinum, and exploded.

In one experiment, 1 ½ grain of the powder (containing particles of paper from the filter,) was put between two flat slips of sheet copper, they were bound together by strong copper wire, and to render them more secure, the ends of the upper slip were made to lap closely over those of the under slip. They were then put on the ring of a brass stand, on the floor, and a lighted taper was placed so as to communicate in a gradual manner a sufficient degree of heat to explode the powder. In about four minutes this effect took place. The report was very loud; the slips were thrown on a high table at a distance of several feet; the wire still held them loosely together, but both were considerably depressed, and especially the lower slip, which in two places, to the extent of half an inch, had its parts folded, the one over the other. These experiments, though on a very limited scale, are sufficient to prove that the powder is capable of exerting a very considerable power, when fired by heat. Its explosive force appears to be exerted in all directions, but principally downwards. By thoroughly drying the powder, and gradually raising it to the temperature at which it explodes, its fulminating property is very much increased. Before it explodes, its colour varies, and from being brown, it becomes almost black. At the instant of the explosion a flash of light is perceived, and the powder is totally dispersed. In all these circumstances, there is a very near coincidence between fulminating platinum and fulminating gold, and I shall again

have occasion to notice other points of resemblance in their properties.

The temperature at which fulminating platinum explodes, appears to be about 400° Fahrenheit. When it was placed on a surface of mercury heated to 420°, it instantly exploded. When the thermometer stood at 410°, a short interval elapsed before this effect took place. In two instances I succeeded in exploding it at 400°. From some comparative trials, I found fulminating gold exploded in similar circumstances, but I could not succeed with it at a temperature below 406°. I am inclined to think, both fulminating powders, if dried at the same temperature, will be found to explode nearly at the same degree of heat. Under certain circumstances, both of the fulminating compounds appear to lose their explosive property, and to be quietly decomposed. This was observed, in some instances, when they were placed on mercury at a temperature varying from 300° to 380°, and a very short interval suffered to elapse before the heat was raised. It was not possible, then, to explode them at any temperature. In other cases, however, in which there was a similarity of circumstances, both powders exploded. These results, though apparently capricious, or even opposite, may perhaps admit of some explanation. The mercury on which the experiments were tried was impure, and exhibited by heat a tarnished surface, from partial oxidation. Hence, in the foregoing experiments, the fulminating powders exploded in some cases, as when placed on an oxidated surface, because the heat did not call into play any other affinities than those existing between the elements of the compounds. But the powder did not explode in other instances; as when in contact

with a surface of mercury, the affinity of this metal interfered, and it slowly formed amalgams with the metallic part of the powders. This view of the facts seems to derive additional evidence from the circumstance, that in cases when no explosions occurred, the powders remained stationary, and gradually acquired a whitish colour from amalgamation.

I tried to explode the powder by friction, and did not at first succeed, but on well drying it, and warming the vessels in which the experiment was to be made, I was able to explode it, both in a Wedgwood and steel mortar. The effect was feeble, and consisted merely of a few slight cracks. Fulminating gold is much more readily exploded in this way. I was unsuccessful in my attempts to explode fulminating platinum by percussion.* The powder did not appear to conduct electricity, when tried with a power of forty plates of four inches square, charged so as to burn thin iron wire; but when the spark was taken from two metallic surfaces in the vicinity of the powder, a few particles of it exploded and produced a red light. I afterwards well dried some fulminating platinum and gold, and when a battery of two hundred plates of four inches square was in good action, so as readily to burn the different metals, I could not succeed in exploding either of the powders. Fulminating mercury was instantly fired under the same circumstances. When the charge of a Leyden battery of fifteen large jars was passed through a quarter of a grain of fulminating platinum, most

* I was unable also to explode fulminating gold by percussion. I made the attempt with both powders, in a steel mortar warmed before the fire. The steel pestle was also warm, and the powders previously well dried.

of it was dispersed, but there was a slight effect with the appearance of red light.

Fulminating platinum is tasteless, and insoluble in water. It is not affected by this fluid at any temperature. When exposed to air in a dry state it acquires a little moisture, but this effect is very limited. Twenty-two grains when well dried at a temperature of about 212°, and made to expose a large surface to the atmosphere for two days, gained half a grain, but there was no farther increase on exposing the powder for two days longer, and when gradually heated to the temperature at which it had been dried, it weighed twenty-two grains, as at first.

I have not ascertained the specific gravity of the powder, but from the greater apparent bulk of an equal weight of it, as compared with that of fulminating gold, I conceive its specific gravity is less than this last substance.

Fulminating platinum is soluble in cold sulphuric acid, but much more readily so by the assistance of heat. The solution is of a dark red brown colour. It is less soluble in muriatic and nitrous than in sulphuric acid. There seemed to be a slight spontaneous action, and a disengagement of gas, when each of the above acids was brought in contact with the fulminating powder. At first, I thought carbonic acid might exist in the powder, and the manner in which it was prepared, did not necessarily preclude the presence of that substance. To ascertain if this were the case, or whether any gas were disengaged by the agency of an acid, I put two grains of the powder in a cubic inch, filled it with mercury, and inverted it over a mercurial trough; half a cubic

inch of sulphuric acid diluted with $\frac{1}{10}$ its volume of water was let up into the tube, but there was no disengagement of gas, the acid slowly dissolved the powder, and the solution was of a very dark colour.

Chlorine has no spontaneous action on the fulminating powder, but on the application of heat it is decomposed, white fumes are disengaged, a whitish brown sublimate of muriate of ammonia and a dark coloured muriate of platinum are produced. The powder is not affected by pure liquid ammonia. This alkali may be boiled off from it without impairing its fulminating properties.

The powder was not apparently affected when suffered to remain for twelve hours in a retort filled with ammoniacal gas, nor did any change take place by the application of a gentle heat; but when the temperature was increased, there was a succession of slight explosions, the powder was decomposed, and the metal reduced.

When the powder was put into a retort filled with muriatic acid gas, there was a slight spontaneous action; on the application of heat, there was a feeble crack, the powder was decomposed, white vapours of muriate of ammonia were disengaged, and muriate of platinum was formed, which deliquesced by exposure to the air. The effects were similar when fulminating gold was treated in the same way.

Alcohol has no action on the powder. When mixed with flowers of sulphur and heated in a small retort, the powder is quietly decomposed, and sulphuret of platinum is obtained.

When the powder is brought in contact with phosphorane, a hissing noise is produced, the powder appears to be par-

tially decomposed, and muriatic and phosphoric acids are probably formed. Fulminating gold is affected in a similar way by phosphorane.

IV. *Composition of Fulminating Platinum.*

From the manner in which fulminating platinum was obtained, it was not difficult to form conjectures concerning its constitution. As it was furnished by the agency of potash or soda on the ammonia sulphate of platinum, it was easy to conceive it might be composed of oxide of platinum and ammonia. The composition of aurum fulminans, and the analogies existing between this substance and fulminating platinum, were favourable to this idea; but the experiments I made afforded me more direct and satisfactory evidences on the subject, and confirmed the opinion I had previously formed of its nature. Thus, when the powder (previously well dried,) was put into small green glass tubes filled with dry mercury and exploded by heat, a quantity of gas was obtained, which had the properties of nitrogene; moisture lined the sides of the tubes, and the mercury formed an amalgam with the platinum.

When the powder was mixed with quicklime, and the mixture heated in a small retort, it was decomposed with a few slight cracks; a little fluid condensed in the neck of the retort; it had the smell of ammonia, and instantly rendered turmeric paper brown; a little gas also came over, having the properties of nitrogene. The presence of moisture appeared to be necessary in the foregoing experiment to develope the ammonia; for when the powder was mixed with quicklime, previously heated to redness, the slight explosions arising

from its decomposition were more numerous, and the odour of ammonia could not be perceived. When, however, a few drops of water were added to the mixture of dried quicklime and fulminating powder, ammonia was produced by the application of heat. When the powder was put into a small retort with pure muriatic acid, the fluid boiled to dryness, and the dry mass heated to redness, the platinum remained in the metallic state, and a whitish sublimate was deposited in the neck of the retort, which, when collected and mixed with quicklime, spontaneously evolved ammonia.

When nitrous acid was boiled to dryness on the powder, and the heat continued, a quantity of gas was obtained, which appeared to be nitrous oxide, as it enlarged the flame of a taper and was absorbed by water; and oxide of platinum alone remained in the retort.

The foregoing results appear to furnish satisfactory evidences as to the nature of fulminating platinum, that it is a compound of oxide of platinum and ammonia. From the following experiments, I shall venture to deduce the proportions of its constituent parts. In the analysis, I employed muriatic acid and sulphur to ascertain the quantity of platinum, and nitrous acid to determine the proportion of oxide in the fulminating compound; and the coincidence between results obtained by such different methods, affords strong presumptions as to the accuracy of the experiments.

Exp. 1. Ten grains of the fulminating powder were put into a small retort with pure muriatic acid, the retort was heated over water so as to boil the fluid to dryness and decompose the dry mass; no permanently elastic fluid came over, except the common air in the retort; a grey substance sublimed,

that had the taste of sal ammoniac, and readily afforded ammonia when mixed with quicklime. The presence of ammonia was not only indicated by the odour, but by the instant production of white fumes when muriatic acid was brought near the mixture; and turmeric paper was immediately changed to brown by the ammonia disengaged. The bulb of the retort contained the platinum; it was of a white colour, and appeared to be quite reduced. But to secure this effect, the bulb containing the platinum was put into a small Hessian crucible, and exposed to a dull red heat. The metal was then carefully separated from the bulb, and exposed to a full red heat in a platinum crucible, when it weighed $7\frac{3}{8}$ grains = 7.375 grains.

Exp. 2. Ten grains of the powder treated precisely as in the preceding experiment afforded 7.3 grains of platinum.

Exp. 3. Five grains of the powder were well mixed with rather more than an equal bulk of flowers of sulphur. The mixture was exposed to a dull red heat in a small retort, and furnished a black sulphuret of platinum, which when decomposed at a red heat in contact with the atmosphere afforded $3.\frac{11}{16}$ grains = 3.6875 grains of platinum.

Now, according to these experiments, the first and third of which exactly agree, 100 grains of the fulminating powder contain 73.75 grains of platinum; for 10 : 7.375 :: 100 : 73.75.

Exp. 4. Ten grains of the powder were put into a small retort with pure nitrous acid. The acid was boiled to dryness, nitrate of ammonia was formed and yielded nitrous oxide gas by its decomposition. The retort after being exposed to a dull red heat contained 8.25 grains of a shining dark grey substance, which I have found is a pure oxide of

platinum. It is decomposed at a full red heat, and yields only oxygene and platinum. I presume this compound has not yet been described; an account of it I hope to have the honour of shortly laying before the Society.

Exp. 5. Ten grains of the powder, after being decomposed by the agency of nitrous acid, as in the preceding experiment, afforded 8.5 grains of dark grey oxide of platinum. But a little of it appeared to be damp; it was put into a small dry retort and exposed to a dull red heat; nitrous acid vapour appeared in the neck of the retort, but no gas was expelled. Whilst the retort was yet warm the bulb was taken off, and the oxide when carefully collected weighed 8.25 grains. From the two last experiments it appears that 100 grains of the fulminating powder contain 82.5 grains of oxide of platinum, for 10 : 8.25 :: 100 : 82.5.

In one experiment in which I decomposed the dark grey oxide of platinum at a full red heat, 10 grains afforded me 8.82 grains of platinum. In another instance, 7 grains of the oxide yielded 6.187 grains of platinum. If the mean of these experiments be taken, 100 grains of the oxide will contain

88.3	platinum
11.7	oxygene
100.0	

If the results obtained in the five preceding experiments be compared with the subsequent ones on the composition of the grey oxide of platinum, they lead to the conclusion, that the platinum in the fulminating powder is in the same state of oxidation as the oxide directly procured from the powder by the agency of the nitrous acid. There is indeed a slight

difference in the composition of the oxide, as deduced from the experiments with the nitrous and muriatic acids, compared with those on the immediate decomposition of the oxide by heat. But as this difference is only about 1 per cent. of oxygene greater by the latter than the former methods, there can be no ground to suppose the state of oxidation different in either case.

The quantity of platinum and of oxygene, or of oxide of platinum, in the fulminating compound, having been determined, it remained to ascertain the proportion of ammonia in the powder. With this object in view, I made a number of experiments on the decomposition of the powder by the agency of heat in close vessels. I first used small green glass retorts containing the powder and filled with recently boiled mercury; but I found in two trials the retorts would not stand the shock from a single grain, but snapped off at the neck at the instant of the explosion, owing perhaps to the principal explosive force being directed downwards. I then had recourse to straight tubes, varying in length from nine to eighteen inches, and in diameter of bore from one-third to half an inch. The quantity of the powder I used in these tubes was from half a grain to two grains. The experiments were made in this way: the powder being placed in the tube, it was held in an oblique direction, and filled with dry mercury, so that when inverted in a vessel of mercury, all the powder remained nearly at the top of the tube. The tube was then fastened to a brass stand, in an inclined position, and heat sufficient to explode the powder was communicated by means of red hot balls, or by a spirit lamp. In a number of trials made in this way there was no instance in which the whole

of the powder exploded at once; it went off at intervals, as its particles reached the proper temperature, without producing much noise, though the gas generated was driven down the tubes with sufficient violence to force, in most cases, some of the powder out of the tubes. This mode of operating seemed to promise more accurate results than any other that occurred to me. The products, in cases when the tubes did not break, were the same, viz. a quantity of gas, aqueous moisture, and platinum in alloy with mercury. But the quantity of gas varied in most instances, owing to the difficulty of exploding the whole of the powder. In two cases, however, I obtained corresponding results. In one of these experiments a grain of the powder, after being dried at about 212°, and decomposed in contact with dry mercury, afforded 0.18 of a cubic inch of gas; in the other, half a grain of the powder furnished, under similar circumstances, 0.09 of a cubic inch of gas. But in both of these experiments, it is proper to remark, a minute portion of the powder had been thrown out of the tubes by the explosions. This was evident on inspecting the surface of the mercury, and from the slight explosions which took place when a heated iron was brought near.

Though the preceding experiments could furnish no data for determining the exact proportion, they might at least afford approximations to the true quantity of ammonia in the fulminating powder; and this consideration induced me to examine the gas I procured with some attention. I shall briefly state the particulars of one examination of this kind, the barometer being at 30°, and the thermometer at 60°. The permanent gas obtained from one grain of the fulminating

powder, when standing over mercury; occupied 0.18 of a cubic inch. When it was transferred to pure water and agitated, it diminished to 0.15 of a cubic inch. An equal volume of pure nitrous gas being added to the 0.15, there was no sensible diminution. After the nitrous gas had been absorbed by a fresh solution of green sulphate of iron, the residual gas immediately extinguished a lighted taper. These experiments corresponded with others I had previously made, and they all seemed to prove that the gas produced during the decomposition of the fulminating powder is for the most part nitrogene. The appearance of gas absorbable by water, was at first rather unexpected. I supposed this gas might be ammonia, and this opinion acquired additional probability from calculations derived from the results of my experiments; but I soon convinced myself from actual trials, that ammoniacal gas is disengaged during the decomposition of fulminating platinum by heat.

I attempted to explode five grains of the powder in a strong green glass tube, two feet in length, and two-thirds of an inch in diameter of bore. I succeeded in exploding a sufficient quantity of the powder to furnish a half cubic inch of gas; but in cooling, the tube cracked and the mercury fell. I immediately examined the tube. The space occupied by the gas was lined with a thin coat of moisture. The odour of ammonia was very perceptible in the tube, and turmeric paper was changed to brown by the moisture in it.

The fact of the disengagement of ammoniacal gas on exploding the powder, seems also to be proved by a very simple experiment I made. I put a little of the powder in the centre of a tube about eighteen inches in length. I opened

a bottle of strong muriatic acid, and placed it at the open end of the tube. I then exploded the powder by the heat of the spirit lamp; at the instant of the explosion, a quantity of dense white vapour, like muriate of ammonia, made its appearance.

The effects are similar, when aurum fulminans is treated in the same way. I have mentioned the appearance of moisture in cases when fulminating platinum was exploded in close vessels, after being well dried; and it is proper to state, that the uniform exhibition of water in such circumstances, in much greater quantity than could be formed in the experiments, leads to the conclusion that this fluid is one of its constituent parts.

From the statements that have been made, it appears that fulminating platinum is a triple compound, consisting of oxide of platinum, ammonia, and water. The experiments already detailed seem to prove that 100 grains of the powder contain,

Of	platinum -	73.75
	oxygene -	8.75
	ammonia and water	17.50
		100,00

or of	oxide of platinum	82.5
	ammonia and water	17.5
		100.0

Approximations to the respective quantities of ammonia and water, in 100 grains of fulminating platinum, may be gained from calculations made on the results furnished in one of the previous experiments given in detail. In the experi-

ment to which I allude, one grain of the powder afforded 0.18 of a cubic inch of gas, 0.15 of which had the properties of nitrogene, and 0.03 appeared to be ammonia. This, however, is not to be considered as the true quantity that a grain would yield, for reasons already stated; but even on this calculation, 100 grains of the powder would furnish 15 cubic inches of nitrogene, and 3 of ammonia.

15 cubic inches of nitrogene weigh about	4.42 grs. and require
45 cub. in. of hydrogene (to form ammonia)	1.01
3 cubic inches of ammoniacal gas	0.54
	5.97 of ammonia in

100 grains of the powder, calculating only from the quantity of gas actually obtained, without taking into account the quantity of ammoniacal gas absorbed by the water present, which must have been saturated with this gas. The water arose from two sources;—it formed a constituent part of the powder, and it was generated from its elements during the explosion. The quantity of water formed would be 8.66 grains. The 1.01 grain of hydrogene would require for this purpose 7.65 grains of oxygene.* I found that $8\frac{1}{4}$ grains of water absorbed about one grain of ammoniacal gas, the thermometer being at 60°, and barometer at 30°; consequently, the 8.66 grains of water generated from 100 grains of the powder, would take up rather more than a grain of ammonia. And if we suppose the water contained in the powder to amount to about $8\frac{1}{2}$ per cent., (and it can scarcely be more,)

* In estimating the weights of the respective gases, and the proportions in which they combine, I have adopted the statements of Sir HUMPHRY DAVY in his "Elements of Chemical Philosophy."

this quantity would also absorb something more than a grain of the alkali.

Hence, to the quantity of ammonia already obtained from calculations derived from an actual experiment, viz. 5.97 grains per cent., which in round numbers may be called 6 grains per cent., we must add 2 grains per cent. for the alkali absorbed by the water present; and making a slight allowance for deficiencies arising from the minute portion of the powder that escaped decomposition in the experiment on which these calculations are founded, I do not think the ammonia in the powder can be estimated at less than 9 grains, and the water at 8½ grains per cent.

On these estimates, 100 grains of fulminating platinum will consist of oxide of platinum 82.5

will consist of oxide of platinum	82.5
Ammonia -	9.0
Water - -	8.5
	100.0

V. *Theory of the formation and decomposition of Fulminating Platinum.*

From the statements made in the preceding pages, fulminating platinum appears to be composed of oxide of platinum, ammonia, and water. It is formed, as has already been stated, in cases when the ammonia-sulphate of platinum is boiled in a solution of fixed alkali. The theory of its formation is apparently simple, and is founded on the superior affinity of the fixed alkalies, over ammonia and oxide of platinum, for sulphuric acid. An examination of the circumstances con-

nected with the formation and decomposition of the fulminating powder will, I presume, prove the correctness of this opinion.

When the triple compound of oxide of platinum, sulphuric acid and ammonia, is boiled in a solution of potash or soda, some ammonia is expelled, fulminating platinum is formed, and the residual fluid affords a copious white precipitate with the soluble salts of barytes and lead; and if it is evaporated nearly to dryness, crystals of sulphate of potash or soda are obtained. These results appear to be connected with the following changes. The triple compound is decomposed, its sulphuric acid unites with the fixed alkali, the ammonia it contains being in greater quantity than can combine with the oxide of platinum, is in part expelled; the remainder enters into a more intimate union with the oxide, and thus fulminating platinum is formed.

In the decomposition of fulminating platinum by heat in close vessels, nitrogene and ammoniacal gases, platinum and water appear to be the only products; and these results correspond with those derived from the agencies of muriatic and nitrous acids on the powder.

From the close analogy that exists between fulminating platinum and fulminating gold in their properties and constitution, they must be regarded as belonging to the same class of bodies, and the theory which explains in a satisfactory manner the decomposition and fulmination of the latter, will likewise serve to account for similar changes in the former. Whilst the composition of fulminating gold was unknown, various explanations as to the cause of its explosive properties

were given by the early chemists; but they were in general, as might have been expected, so vague, and so obscurely developed, as to be at best but unintelligible enigmas. The researches of Bergman fully exposed and refuted all previous opinions on the subject, and led to just views concerning the nature of aurum fulminans. He stated it to be composed of calx of gold and volatile alkali, and he referred its fulmination to the decomposition of the alkali, and the great report and violent explosion to a copious and instantaneous eruption of elastic fluid violently striking the air.* The theory of its decomposition was explained by M. Berthollet in a still more satisfactory manner, when the composition of ammonia was discovered. On this theory, when the fulminating gold is exploded, ammonia is decomposed, its hydrogene unites to the oxygene of the oxide to form water, the gold is reduced, and nitrogene gas disengaged. The sudden expansion of the air and vapour generated in the process, are circumstances connected with the detonation. The decomposition of fulminating platinum admits of a similar explanation; but with the changes already enumerated, we must connect the disengagement of ammoniacal gas, and the instantaneous conversion of the water of composition in the fulminating powder into steam, and these effects are probably intimately connected with the exhibition of its fulminating properties.

* Physical and Chemical Essays, Vol. II.

Cork, December 20, 1816.

XI. *On the parallax of the fixed stars.* *By* John Pond, *Esq. Astronomer Royal, F. R. S.*

Read February 20, 1817.

It is now very generally known to astronomers, that, for several years past, Dr. Brinkley, with the eight feet circle of the Observatory at Dublin, has constantly observed a periodical deviation of several fixed stars from their mean places; which strongly indicates the existence of an annual parallax in those stars. The magnitude and perfection of the instrument, the regular continuation of the same result without exception, during a period of several years, and above all, the judicious reflections of Dr. Brinkley, and his unprejudiced statement of every objection that might be supposed to occur, seem to leave but little doubt upon the subject. Thus much at least is certain, that the observed discordance arises from some very permanent cause, and is totally distinct from what has usually been termed *error of observation.*

The deviation from the mean place resulting from the supposed parallax being a very small quantity (never exceeding a single second), Dr. Brinkley was naturally desirous that the result of his observations should be confirmed by other astronomers. Few, however, are fortunate enough to possess instruments sufficiently accurate either to confirm or confute the hypothesis above stated.

The mural circle at the Royal Observatory, erected in the

year 1812, was supposed to be well adapted to this species of investigation, and, I confess, I expected to find the effects of parallax in Dr. Brinkley's stars, and perhaps in some others, almost as decidedly as the effects of aberration and nutation. I soon however found (what indeed if I had sufficiently reflected on the subject, I ought to have foreseen), that this instrument (at least in the manner in which I employ it) is not so exactly adapted to the purpose as might at first be supposed. My principal object was to obtain the mean places of a certain number of stars, with the greatest precision that the nature of the instrument admitted. I therefore availed myself of the principles of its construction, to give every possible variety to my observations, by bringing a new system of divisions to bear upon those stars. The effect of parallax was necessarily involved in these changes; and, though I certainly did expect that under all these disadvantages a parallax so considerable as that assumed by Dr. Brinkley, would have become very apparent, yet, upon not finding it, I did not think it by any means fair to infer its non-existence, more particularly as the discordances I really did meet with, were very universally in favour of parallax.

Finding, therefore, that I could not elucidate this question in a perfectly satisfactory manner without dedicating the circle entirely to this investigation, I rather directed my attention to contrive some other instruments which might be employed exclusively to this object.

At the last visitation, I proposed that two or more telescopes should be fixed to stone piers, and directed to the particular stars whose parallax was suspected; that each telescope should be furnished with a micrometer, by which the star

could be compared with others passing through the same field. This suggestion was approved of by the visitors, and, till a proper building can be contrived and erected, I have fixed two ten feet telescopes, one on the circle pier directed to α Aquilæ, and one on the quadrant pier directed to α Cygni, and with these temporary instruments I am about to commence a series of observations.

The advantages I presume those instruments to possess are, length of radius; great steadiness, and simplicity of construction; and being used only for a few select observations, these few are more likely to be made with extreme care.

Although, for the reasons above stated, I was unable to devote the mural circle entirely to the investigation of parallax, yet during the summer of 1813, and the following winter, I was induced to continue the telescope in the same position, with the view of examining any changes that might occur indicative of parallax, or any other irregularity. During this period, the three principal stars observed by Dr. Brinkley (α Lyræ, α Aquilæ, α Cygni) arrive at their maximum and minimum of parallax; as far, therefore, as the observations of one single year can be supposed to have any weight, these seem, I think, to be as good as ever may reasonably be expected to be made with the same instrument.

The object of the present communication is to submit the result of these observations to the Society; and whatever remarks I may be induced to make on the discordances between Dr. Brinkley's observations and my own, I hope, will be considered rather in the light of suggestions, arising from circumstances obviously presenting themselves to our notice, than as arguments to decide a question, which

I anxiously wish to keep open for future experiment and investigation.

The question of parallax in a theoretical point of view, is scarcely of any importance, it is in fact one of mere curiosity. The motion of the earth has long since ceased to be a subject of controversy, and could a difference of opinion still be supposed to exist, the advocates for the Copernican system would derive but slender support from the discussion of such small variations, as form the subject of this Paper. But with reference to the state of practical astronomy the case is very different; in the future history of this branch of the science, that period of time will always acquire a certain degree of celebrity, in which astronomical instruments shall have been brought to such a degree of perfection, as to exhibit distinctly the effects of parallax in the fixed stars, and to distinguish these from the variety of complicated oscillations to which, from other causes, they are perpetually subject: and, as far as relates to the natural history of the sidereal system (if I may so express myself), it is surely a subject of rational curiosity to ascertain whether the distance of the nearest fixed star can be numerically expressed from satisfactory data, or if it be really so immeasureably great, as to exceed all human powers either to conceive or determine.

As I have already observed, the object of this communication is not to decide this question, but to state the result of the Greenwich observations.

The first star I shall consider, is α Lyræ; this star having been diligently observed from the first erecting of the instrument with a view to this particular investigation.

In the annexed Tables will be seen the observations selected

at those times when the parallax is at a maximum. To the observations themselves is subjoined the process of computing the correction to be applied for deducing the true polar distance. This correction, which is of the nature of an Index error, is usually found by comparing the observed places of all the stars, during the same period, with their computed places deduced from a standard catalogue derived from the instrument itself.

If all the stars were used indiscriminately for this purpose, the method would be liable to an objection stated by Dr. BRINKLEY, for if of the stars from which this correction is deduced, several were themselves subject to considerable parallax, the effect of this parallax would be involved in the correction, and, if they were selected near to the star whose parallax was sought, the effect of this would be to conceal the parallax, by showing only the difference of parallax instead of the whole. To obviate entirely this objection, I reject those stars supposed by Dr. BRINKLEY to have parallax, and likewise γ Draconis, whose parallax arrives at its maximum nearly at the same period with that of α Lyræ, α Aquilæ, and α Cygni. I employ chiefly those stars whose parallax must be neutral, and those opposite in right ascension, which method has rather a tendency to exaggerate the effect of parallax by exhibiting to a certain degree the sum of the parallaxes of different stars. The difference of these two methods, however, as may be seen in the annexed Tables, does not amount to one tenth part of a second. The abovementioned objection, therefore, though theoretically just, cannot be made to explain the discordance which exists between Dr. BRINKLEY's observations and mine.

	°	′	″
The mean of 40 observations of α Lyræ from June 22, to August 21, gives for the north polar distance of that star - - -	51	23	0,278
The mean of 20 taken nearer to the period of opposition will be - -	51	23	0,461
The mean of 30 winter observations is	51	23	0,867

The discordance between the winter and summer observations, therefore, does not exceed 0,6″ equal to one third of the discordance found by Dr. BKINKLEY, and with the refraction employed by him, it would be about one quarter of a second less.

α Cygni.

	°	′	″
Thirty observations of this star in summer, about the period of opposition, give -	45	22	56,933
The mean of 30 in winter - -	45	22	57,448
But as 10 of these observations were made too far distant from the time of conjunction, it will be better to take the mean of 20, which is - - -	45	22	57,489

The difference 0,556″ is the total discordance in favour of parallax. This quantity is likewise nearly equal to one-third of the discordance found by Dr. BRINKLEY.

α Aquilæ.

		°	′	″
Thirty observations of this star in the summer of 1813, are as follows: -	10=	81	36	58,555
	10=	81	36	58,536
	10=	81	36	58,300
Mean of 30 summer - - -		81	36	58,464
Mean of 20 winter - -		81	36	59,300
Difference - - -				0,836

With Dr. Brinkley's refractions, this difference would be reduced to less than half a second, a quantity equal to one quarter of the discordance found by Dr. Brinkley.

On the Observations of the year 1812.

The observations of the year 1812, are liable to all the objections which I have already stated, yet so very small is the effect of change in the position of the telescope, that I should be inclined myself to prefer a mean taken from the two years observations, than to that deduced from the observations of 1813 above. The early observations in the summer of 1812, are probably not very good, the instrument being then in a very unfinished state. α Lyræ was diligently observed during the whole of the summer; there are but few observations of α Aquilæ, and none of α Cygni; but the winter observations of these stars are very good. The mean result of two years observations will stand as follows:

α Lyræ.

	Summer No. of Observations.	"		Winter No. of Observations.	"
1812	24	0,165	1812	20	0,711
1813	40	0,278	1813	30	0,873
	64	0,236		50	0,808

Difference 0.572" with Bradley's refractions.

The French refractions would reduce this difference to about 0,3" or 0,4".

α Aquilæ.

	Summer No. of Observations.	"		Winter No. of Observations.	"
1812	10	58,052	1812	20	59,606
1813	30	58,464	1813	20	59,300
	40	58,361		40	59,453

Difference 1.092"

This quantity by the French refraction employed by Dr. BRINKLEY, would be reduced to about 0,5″.

α Cygni.

Winter Observations.				Summer Observations.	
9	1812	57,120″			
20	1813	57,489	1813	30	56,933″
29		57,366			
Difference		0,433			

From the above observations then, it appears that in the three stars supposed by Dr. BRINKLEY to have the greatest parallax, viz. α Lyræ, α Cygni, and α Aquilæ, we find discordances between the summer and winter observations of nearly half a second; now, though these quantities are so much smaller than those found by Dr. BRINKLEY, they appear to me to be equally independent of any accidental error of observation, and it is not improbable but they may originate from some similar cause. But, I confess, I doubt much if this cause be parallax, and for the following reasons.

I do not find these discordances sensibly increased by direct comparison with an opposite star as Capella; now the maximum of parallax is nearly as great in Capella, as in α Aquilæ, at least in the proportion of 4 to 5; it is very unlikely then, the parallax of the one star should be above 5″, and the other an insensible quantity. It may be remarked, that both Dr. BRINKLEY and myself, find nearly the same discordance in all these stars, though the deduced parallax necessarily comes out very different. This is very unlikely to happen from parallax. Moreover, these stars all pass the meridian about the time of the winter solstice at their maximum of parallax,

and, in proportion as stars do not possess this property, both Dr. BRINKLEY and myself find either a much smaller discordance, or none at all.

It is rather, therefore, to this peculiar circumstance that we should look for some explanation of the difficulties.

In addition to this I should add, that I find γ Draconis involved in this discordance, α Lyræ and γ Draconis have been observed together for five successive years. Above three hundred observations of each star have been made in opposition, and as many in conjunction, and I find the difference of parallax from the mean of all these observations to be about 0,25″, which quantity by the French refraction would be reduced one-half, or to an insensible quantity.

Now it is quite improbable, that two stars of such different brightness should have so exactly the same parallax.

These arguments, however, though they seem to me to arise naturally from the data before us, are nevertheless by no means absolutely conclusive, and I am well aware, how much easier it is to suggest doubts, than to propose satisfactory explanations. I shall, therefore, conclude this Paper, by expressing my hopes that, before a very long period shall elapse, the instruments lately erected may afford something more satisfactory and decisive.

Method of computing the Equation or Index Error to be applied to the observed North Polar distances.

1813. July and August. Position 0°

	No. of Observations.	North Polar distances by circle.	North Polar distances by Catalogue.	Deduced Index Error.	Index Error multipled + No. of Observations.
		° ′ ″	″	″	″
Polaris, SP -	9	358 18 38,98	38.35	+ 0.63	+ 5.67
β Urs. Min. - -	27	15 4 49,31	48.93	0.38	10.26
β Cephei - -	14	20 15 31,50	30.54	0.96	13.44
α Urs. Maj. -	12	27 14 31,83	31.50	0.33	3.96
α Cephei - -	13	28 12 12,86	12.55	0.31	3.93
γ Urs. Maj. - -	8	35 15 55,69	55.25	0.44	3.52
γ Draconis -	39	38 29 3,96	3.55	0.41	15.99
η Urs. Maj. -	19	39 44 58,31	57.81	0.50	9.50
α Persei - -	6	40 49 53,10	52.37	0.73	4.38
Capella - -	27	44 12 20,64	20.46	0.18	4.86
α Cygni - -	23	45 22 57,30	56.98	0 32	7.36
α Lyræ -	39	51 23 0,70	0.48	0.22	8.58
Castor - -	6	57 42 47,12	46.74	0 38	2.28
Pollux -	11	61 31 56,63	56.42	0.21	2.21
β Tauri - -	17	61 33 44,46	43.67	0.79	13.43
α Cor. Bor. -	25	62 38 56,04	55.45	0.59	14.75
α Arietis, - -	5	67 25 36,64	36.51	0.13	0.65
Arcturus -	22	69 50 19,23	19.00	0.23	5.06
Aldebaran -	12	73 52 35,61	35.26	0.35	4.20
α Herculis - -	20	75 23 14,45	13.92	0.53	10.60
Regulus - -	3	77 47 22,65	22.62	0.03	0.09
α Ophiuchi -	33	77 17 39,07	38.88	0.19	6.27
α Aquilæ -	29	81 36 58,79	58.79	0.00	0.00
α Orionis - -	14	82 38 15,64	15.65	0.00	0.00
α Serpentis -	26	82 58 39,81	39.23	0.58	15.08
No. of Observations	459			0,36	166.07
Rejecting γ Draconis, α Lyræ, α Aquilæ, α Cygni	329			0.41	134.14

Method of computing the Equation or Index Error to be applied to the observed North Polar distances.

1813. November.

		° ′ ″	″	″	″
Polaris -	13	1 41 23.38	21.65	+ 1.73	+ 22.49
SP. - -	12	358 18 40.90	38.35	2.55	30.60
β Urs. Min. -	10	15 4 51.04	48.93	2.11	21.10
β Cephei - -	10	20 15 32.09	30.54	1.55	15.50
α Cephei -	10	28 12 14.16	12.55	1.91	19.10
α Cassiop. - -	10	34 29 24.30	22.68	1.62	16.20
γ Urs. Maj. -	4	35 15 56.81	55.25	1.56	6.24
γ Draconis -	7	38 29 6.21	3.55	2.66	18.62
η Urs. Maj. - -	10	39 45 0.00	57.81	2.19	21.90
α Cygni -	8	45 22 59.31	56.98	2.33	18.64
α Lyræ - -	10	51 23 2.52	0.48	2.04	20.40
α Andromed. -	14	61 56 32.02	30.16	1.86	26.04
α Arietis -	11	67 25 38.89	36.51	2.38	26.18
Arcturus -	11	69 50 21.01	19.00	2.01	22.11
α Ophiuchi -	3	77 17 40.74	38.88	1.86	5.58
α Aquilæ -	10	81 37 1.30	58.80	2.50	25.00
	153			2.06	315.70
Rejecting γ Draconis, α Lyræ, α Aquilæ, α Cygni	118			1.97	233.04

1813. December.

		° ′ ″	″	″	
Polaris - -	7	1 41 24.40	21.65	+ 2.75	19.25
β Urs. Min. -	3	15 4 52.41	48.93	3.48	10.44
β Cephei - -	2	20 15 31.88	30.54	1.34	2.68
α Cephei -	3	28 12 15.88	12.15	3.73	11.19
α Cassiop. - -	8	34 29 25.56	22.68	2.88	23.04
γ Draconis -	5	38 29 7.16	3.55	3.61	18.05
α Persei -	5	40 48 54.85	52.37	2.48	12.40
Capella -	2	44 12 23.17	20.46	2.71	5.42
α Cygni - -	4	45 23 0.42	56.98	3.44	13.76
α Lyræ - -	6	51 23 4.21	0.48	3.73	22.38
α Andromed. -	2	61 56 31.90	30.16	1.74	3.48
α Arietis - -	7	67 25 39.63	36.51	3.11	21.77
Arcturus -	2	69 50 22.10	19.00	3.10	6.20
Aldebaran -	6	73 52 37.92	35.26	2.66	15.96
α Aquilæ - -	4	81 37 2.09	58.80	3.29	13.16
	66			3.02	199.18
Rejecting γ Draconis, α Lyræ, α Aquilæ, α Cygni	47			2.81	131.83

Method of computing the Equation or Index Error to be applied to the observed North Polar distances.

1814. January.

Polaris - -	5	1 41 5.30	2.00	+ 3.30	16.50
α Cassiop. -	5	34 29 6.70	2.85	3.85	19.25
α Persei -	6	40 48 42.02	38.87	3.15	18.90
Capella -	6	44 12 19.40	15.89	3.51	21.06
β Tauri -	4	61 33 43.10	39.84	3.26	13.04
α Andromeda -	4	61 56 14.99	10.17	4.82	19.28
α Arietis - -	5	67 25 22 34	19 11	3.23	16.15
Aldebaran -	4	73 52 30.83	27.31	3.52	14.08
	39			3.55	138.26

1814. February.

Polaris - -	11	1 41 5.40	2.00	+ 3.40	37.40
α Cassiop. -	9	34 29 6.52	2.85	3.67	33.03
α Persei -	12	40 48 42.30	38.87	3.43	41.16
Capella -	20	44 12 19.50	15.89	3.61	72.20
Castor - -	14	57 42 57.80	53.80	4.00	56.00
Pollux - -	12	61 32 8.50	4.42	4.08	48.96
β Tauri -	16	61 33 43.86	39 84	4.02	64.32
α Arietis - -	8	67 25 22.80	19.11	3 69	29.52
Aldebaran -	10	73 52 31.13	27.31	3.82	38.20
α Orionis -	16	82 38 18.18	14.28	3.90	62 40
	130			3.97	492.95

α Lyræ.

1813.	A	B	C	D
	° ′ ″		″	
June 22	51 22.59.64	−0.40	59.24	
23	23. 1.49		1.09	
24	0.87		0.47	
25	0.78		0.38	
26	1.13		0.73	0.549
27	1.34		0.94	
28	0.54		0.14	
July 5	0.73		0.33	
6	1.67		1.27	
9	1.30		0.90	
10	1.16		0.76	
11	0.26		59.86	
12	1.11		0.71	
13	0.17		59.77	
16	0.93		0.53	
17	1.86		1.44	0.374
18	1.80		1.40	
19	1.25		0.85	
23	22.59.28		58.88	
24	59.94		59.54	
25	59.67		59.27	
27	23. 0.72		0.32	
28	0.07		59.67	
29	22.59.80		59.40	
30	59.40		59.00	
Aug. 1	59.53		59.13	59.877
3	23. 0.18		59.78	
5	1.24		0.84	
7	1.50		1.10	
9	0.66		0.26	
10	0.75		0.35	
11	0.62		0.22	
12	0.33		59.93	
13	0.84		0.44	
15	0.69		0.29	0.313
16	0.51		0.11	
17	0.72		0.32	
19	1.43		1.03	
20	1.04		0.64	
21	0.20		59.80	

1813.	A	B	C	D
	° ′ ″		″	
Nov. 4	51 23. 2.00	−2.00	0.00	
6	1.31		59.31	
8	2.09		0.09	
11	2.50		0.50	
18	3.85		1.85	0.688
20	2.33		0.33	
27	2.90		0.90	
30	3.49		1.49	
Dec. 15	4.65	−2.80	1.85	
21	3.36		0.56	
23	3.24		0.44	
26	4.57		1.77	
30	4.31		1.51	
31	5.12		2.32	
1814.				
Jan. 11	4.10	−3.55	0.55	1.111
12	5.03		1.48	
29	4.90		1.35	
30	4.58		1.03	
31	3.87		0.32	
Feb. 2	4.14	−3.80	0.34	
3	3.81		0.01	
6	3.93		0.13	
14	3.73		59.93	
17	5.27		1.47	
19	5.70		1.90	0.801
20	5.11		1.31	
22	4.38		0.58	
24	4.58		0.78	
25	4.90		1.10	
26	4.60		0.80	

A. Observations given by the instrument corrected for precession, Abb. Nut. and Refraction.

B. Index Error.

C. Observations corrected.

D. Means of each series of 10 observations.

Cygni.

1813.	A	B	C	D
July 27	45° 22′ 56″.68	−0.40	56″.28	
28	57.84		57.44	
29	57.75		57.35	
30	57.93		57.53	
Aug. 3	57.56		57.16	56.791
4	57.70		57.30	
5	56.68		56.28	
7	57.71		57.31	
9	56.09		55.69	
10	55.97		55.57	
11	57.43		57.03	
12	57.72		57.32	
13	58.28		57.88	
15	57.70		57.30	
17	55.98		55.58	
19	56.84		56.44	56.987
20	57.53		57.13	
21	56.82		56.42	
22	57.54		57.14	
23	58.03		57.63	
25	57.34		56.94	
26	57.77		57.37	
31	57.58		57.18	
Sep. 2	57.61		57.61	
3	58.29		58.29	
4	56.64		56.64	57.025
5	56.87		56.87	
6	56.12		56.12	
7	56.69		56.69	
9	56.54		56.54	

1813.	A	B	C	D
Oct. 22	45° 22′ 57″.33	−0.88	56″.45	
25	58.01		57.13	
29	58.17		57.29	
31	57.88		57.00	
Nov. 6	58.46	−2.00	56.46	57.095
9	59.11		57.11	
11	58.88		56.88	
13	59.39		57.39	
14	59.36		57.36	
15	59.89		57.89	
18	59.52		57.52	
29	59.91		57.91	
Dec. 15	60.32	−2.80	57.52	
26	60.46		57.66	
30	59.66		56.86	
31	61.26		58.46	57.650
1814.				
Jan. 7	60.85	−3.55	57.30	
12	60.93		57.38	
13	61.18		57.63	
17	61.81		58.26	
30	60.10		56.55	
31	60.10		56.55	
Feb. 2	61.43		57.63	
3	61.09	−3.80	57.29	
14	61.33		57.53	57.599
15	62.19		58.39	
17	60.90		57.10	
19	61.76		57.96	
20	62.40		58.60	
21	62.19		58.39	

α Aquilæ.

1813.	A	B	C	D
July 11	81° 36′ 57.91″	−0.40	57.51″	
12	59.57		59.17	
16	58.90		58.50	
17	58.83		58.43	
19	58.18		57.78	
21	58.96		58.56	58,555
22	59.01		58.61	
25	59.88		59.48	
26	58.48		58.08	
27	59.83		59.43	
29	58.22		57.82	
30	59.58		59.18	
Aug. 2	59.98		59.58	
3	59.20		58.80	
7	59.27		58.87	
10	57.88		57.48	58,536
11	59.54		59.14	
12	58.54		58.14	
13	58.51		58.11	
15	58.64		58.24	
16	57.68		57.28	
17	59.30		58.90	
19	58.51		58.11	
21	57.67		57.27	
22	58.52		58.12	58,300
24	59.29		58.89	
25	58.62		58.22	
26	59.01		58.61	
31	59.19		58.79	
Sep. 2	58.81	−0.00	58.81	

1813.	A	B	C	D
Oct. 13	81° 36′ 58.68″	−0.88	57.80″	
14	60.10		59.22	
16	58.45		57.57	
18	60.51		59.63	
19	59.94		59.06	
21	60.00		59.12	58,843
25	58.67		57.79	
31	60.79		59.91	
Nov. 1	60.84	−2.00	58.84	
4	61.49		59.49	
6	61.06		59.06	
11	60.82		58.82	
13	61.06		59.06	
14	61.50		59.50	
18	61.93		59.93	
22	61.17		59.17	59.224
27	61.49		59.49	
30	61.61		59.61	
Dec. 15	61.24	−2.80	58.44	
20	61.96		59.16	
26	62.71		59.91	
31	63.46		60.66	
1814.				
Jan. 30	62.82	−3.55	59.27	
31	62.32		58.77	
Feb. 3	62.12	−3.80	58.32	59.375
15	63.34		59.54	
17	63.96		60.16	
19	62.98		59.18	
20	62.86		59.06	
24	62.68		58.88	

Appendix to Mr. Pond's *Paper on Parallax.*

Read March 13, 1817.

From the month of April 1814, to the present time, the observations have been made with two microscopes only, and not having this subject in view, they generally are not calculated to throw much additional light on this question. But last autumn, being induced to suspect, that the discordance I had met with in favour of parallax, might arise from the difference of temperature in summer and winter being in an opposite state relatively to the interior and exterior thermometer, I endeavoured this winter to keep the interior temperature of the observatory the same as that without, which the extreme mildness of the season rendered very easy to accomplish. It likewise so happens that from the 1st of July last to the present time, the index error of the instrument has suffered no variation. It may, perhaps, have oscillated a small fraction of a second on each side the mean, but not more; so that during this interval, the circle may be considered as having been a fixed instrument, and therefore not liable to any of the objections above stated by Dr. Brinkley. Under these circumstances, the observations, though not made with six microscopes, are very worthy of attention. Those of α Lyræ, γ Draconis, α Cygni, and α Aquilæ, are very numerous, and there does not appear the least indication of any periodical variation whatever; the

extremely small discordance, which is no doubt accidental, happens, in some of the cases, to be in a direction contrary to parallax.

It now only remains to determine, whether the fixed instruments, lately erected for this particular investigation, will confirm the above result.

Months.	Number of Observations	Results in Seconds.	
			α Lyræ.
July	18	48″.54	correct mean of 50 observations 49″.198. Summer.
Aug.	12	49.70	
Sept.	12	49.98	
Oct.	8	48.76	
Nov.	12	49.66	correct mean of 34 observations 49″.205. Winter.
Dec.	10	48.77	
Jan.	9	48.92	
Feb.	3	49.69	
			γ Draconis.
July	16	3″.54	correct mean of 31 observations 3″.776. Summer.
Aug.	7	4.75	
Sept.	8	3.40	
Oct.	7	3.16	correct mean of 33 observations 3″.827. Winter.
Nov.	9	4.37	
Dec.	8	3.25	
Jan.	9	3.95	
			α Cygni.
Sept.	14	17″.67	correct mean of 37 observations 17″.36. Autumn.
Oct.	12	17.01	
Nov.	11	17.38	
Dec.	9	16.73	correct mean of 37 observations 17″.28. Winter.
Jan.	8	17.11	
Feb.	11	17.94	
Mar	9		
			α Aquilæ.
Aug.	12	29″.65	correct mean of 33 observations 30″.29. Summer.
Sep.	14	30.74	
Oct.	7	30.31	
Nov.	16	30.55	correct mean of 45 observations 30″45. Winter.
Dec.	12	30.25	
Jan.	4	30.74	
Feb.	5	30.61	
Mar.			

With the French refractions, there would appear an extremely small discordance of 0,1″ in a contrary direction to the effect of parallax.

XII. *An Account of some fossil remains of the Rhinoceros, discovered by Mr.* Whitby, *in a cavern inclosed in the lime-stone rock, from which he is forming the Break-water at Plymouth. By Sir* Everard Home, *Bart. V. P. R. S.*

Read February 27, 1817.

When Mr. Whitby engaged to superintend this most arduous undertaking, Sir Joseph Banks requested him to examine narrowly any caverns he might meet with in the rock, and have the bones, or any other fossil remains that were met with, carefully preserved.

Mr. Whitby in compliance with this request, in November 1816, sent up to Sir Joseph Banks a box of fossil bones, which are the subject of the present Paper.

Mr. Whitby states the bones to have been found in a cavern, in the solid lime-stone rock, 15 feet wide, 45 feet long, taking the direction into the cliff, and 12 feet deep.

This cavern was filled with solid clay, in which the bones were imbedded, and lay about 3 feet above the bottom of the cavern. The lime-stone quarries of Oreston, in which this cavern was met with, are situated on the south side of Catwater, and about one mile from Plymouth.

When Mr. Whitby began to work this quarry, the rock was 74 feet perpendicular above high water; the bones were found 70 feet below the surface of the rock, and about 4 feet above high water mark. He quarried 60 feet horizontally into the cliff, before he came to the cavern. Before

Mr. WHITBY began to quarry here, 100 feet had been quarried into the cliff, so that 160 feet was the distance between the cavern and the original edge of the cliff; in all other directions the quarries consist of compact lime-stone to a great extent. The workmen came to this cavern by blasting through the solid rock, and at the depth in the rock at which it was met with, the surrounding lime-stone being every where equally strong, and requiring the same labour to quarry it; Mr. WHITBY saw no possibility of the cavern having had any external communication, through the rock in which it was enclosed.

The cavern was quarried within about a foot of its bottom, the lower clay was not all cleared out, but the bottom was sounded by an iron crow, and rock was every where met with.

Many such caverns, Mr. WHITBY says, have been met with in these quarries, and, in some instances, the rock on the inside was crusted with stalactite; but nothing of that kind was met with in the cavern in which the bones were found; so that there is no proof that any opening in the rock from above had been closed by infiltration.

The quarry in which this cavern was met with, is directly opposite the place where Mr. WHITBY lands, every time he visits the quarries, and therefore his attention was more naturally drawn to it than to any of the others; and as, in the contract of quarrying, there are two prices, one for rock, another for clay-earth and rubbish, and two officers attend, one, for the crown, and the other on the part of the contractors, who measure the contents of all caverns that contain clay, or other soft materials, it is only necessary to mention

that these officers state, that the rock surrounding the cavern, was equally hard with the other parts, requiring the same force to blast it, and that the quarrying was paid for accordingly.

The following is a list of the bones sent up by Mr. WHITBY to Sir JOSEPH BANKS; they all belonged to the Rhinoceros; but it will appear in the enumeration that they were parts of the skeletons of three different animals.

The third grinding tooth from behind, on the right side of the upper jaw.

The third grinder from the left side of the same jaw.

The second grinder from behind, on the right side of the lower jaw.

The second grinder from before, on the left side of the upper jaw.

The third or middle grinder on the left side of the lower jaw.

One of the smallest of the anterior grinders.

The upper portion of the radius of the right fore leg.

A portion of the fifth dorsa vertebra.

A portion of the cotyloid cavity of the scapula of the left shoulder.

The upper part of the atlas, with a portion of the articulating surface.

The olecranon of the right fore leg.

The first phalanx of one of the toes of the fore foot.

The lower extremity of the left os humeri.

Upper part of the right os humeri.

The head of the left os femoris.

The lower extremity of ditto.

One of the bones of the carpus of the left foot.

The lower extremity of the right ulna.

The lower extremity of the inside toe of the right foot.

The head of the os humeri.

The upper part of the right femor with the epiphysis separated.

The metacarpal bone of the middle toe of the right fore foot.

All these bones are in the most perfect state of preservation; almost every part of the surface entire, to a degree that I have never seen in specimens of fossil bones. The metacarpal bone is complete except a small injury on one side of it, which it received probably at the time it was dug out of the clay.

The teeth of the Rhinoceros differing in their form as well as structure, from those of every known animal, it was readily ascertained that these fossil teeth belonged to that animal; and it is a circumstance extremely satisfactory, that every one of the portions of bones dug up, possessed some distinguishing character, so as not only to enable me to ascertain the particular bone to which each broken portion belonged, but that character was also sufficiently well marked, to make it clear that the bones belonged to the Rhinoceros.

It was very much in our favour, that the elephant is the only animal whose equality of size could lead us into any mistake on this subject.

The metatarsal bone, the only one which was sent up entire, was immediately recognized to belong to the Rhinoceros, since these bones in that animal are nearly double the length of the same bones in the elephant.

Mr. Brooks, Surgeon, and Teacher of Anatomy in Blenheim Street, has in his collection the skeleton of a Rhinoceros, which is considered to have been the largest ever seen in this country.

I took advantage of Mr. Brooks's kindness, not only to compare all the fragments of these bones, with the entire ones in the skeleton, but also to measure with some accuracy the length and breadth of the metacarpal bone in the fossil state and that in the skeleton, so that we might form some comparative idea of the size of the two animals, to which they belonged.

The skeleton stands 5 feet 8 inches high, the metacarpal bone is $7\frac{1}{8}$ inches long, $2\frac{1}{8}$ inches broad. The metacarpal bone, in a fossil state, is $8\frac{1}{8}$ inches long, and $2\frac{1}{4}$ inches broad.

All the bones appear to have belonged to Rhinoceroses of nearly the same size, except the cotyloid cavity of the left scapula, which evidently was part of the skeleton of a smaller animal, and the olecranon of the right fore leg of one still smaller.

It is deserving of remark, that all the bones found in this cavern belonged to the same species of animal. Great pains were taken to ascertain whether there were any other bones than those sent up to London, but no others were discovered.

Professor Brande, Secretary to the Society, analysed a portion of one of the bones and a portion of one of the teeth. He remarked, that he had never met with fossil bones so purely earthy, and free of extraneous matters.

When the bone was heated, it exhaled scarcely any smell of animal matter, nor had it lost any of its natural whiteness.

It consisted of

60 Phosphate of lime.
28 Carbonate of lime.
2 Animal matter.
10 Water.

The tooth consisted of

78 Phosphate of lime.
8 Carbonate of lime.
8 Extraneous earthy matter.
6 Animal matter, water and loss.

At my request he made at the same time an analysis of a Rhinoceros's tooth found at Brentford, and of the tibia of an Hippopotamus found at the same place, as well as of the rib of the fossil remains of an animal of the fish tribe found at Lyme; an account of the Brentford bones, as well as of the bones at Lyme, has a place in the Philosophical Transactions.

The Rhinoceros's tooth from Brentford contained

70 Phosphate of lime.
6 Carbonate of lime.
20 Extraneous earthy matter.
4 Animal matter and water.

The tibia of the Hippopotamus contained

50 Phosphate of lime.
5 Carbonate of lime.
24 Siliceous earth.
10 Aluminous earth.
4 Oxide of iron.
2 Water.
5 Animal matter.

The fish's rib contained

50	Phosphate of lime.
19	Carbonate of lime.
15	Aluminous earth.
5	Siliceous earth.
.8	Water.
3	Animal matter.

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METEOROLOGICAL JOURNAL,

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OF THE

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BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL

for January, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Jan. 1	8	0	37	45	30,45	Rain this Month 0,733 Inches.	S	1	Hazy.
	3	0	40	49	30,41		S	1	Fine.
2	8	0	32	46	30,26			0	Thick and foggy.
	3	0	35	49	30,14		W	1	Fine.
3	8	0	34	45	30,02		NW	1	Cloudy.
	3	0	40	48	30,10		N	1	Fine.
4	8	0	30	44	30,37		W	1	Cloudy.
	3	0	38	47	30,36		W	1	Cloudy.
5	8	0	35	45	30,18		W	1	Cloudy.
	3	0	40	50	30,17		W	1	Cloudy.
6	8	0	45	47	29,87		W	1	Cloudy.
	3	0	48	52	29,79		W	1	Cloudy.
7	8	0	37	46	29 82		NW	1	Fine.
	3	0	40	46	29,90		NbyW	1	Cloudy.
8	8	0	40	45	29,75		SW	1	Rain.
	3	0	48	49	29,54		W	1	Rain.
9	8	0	43	48	29,44		SW	1	Fine.
	3	0	47	51	29,55		W	1	Cloudy.
10	8	0	43	50	29,39		NW	1	Fine.
	3	0	46	54	29,49		W	1	Cloudy.
11	8	0	48	51	28,92		W	1	Rain.
	3	0	48	53	29,01		W	1	Cloudy.
12	8	0	39	51	29,49		W	1	Cloudy.
	3	0	42	53	29,42		SW	1	Cloudy.
13	8	0	38	50	29,00		W	1	Cloudy.
	3	0	40	52	28,93		W	1	Cloudy.
14	8	0	38	48	29,17		SE	1	Cloudy.
	3	0	42	48	29,28		SE	1	Rain.
15	8	0	38	47	29,34		S	1	Fair.
	3	0	38	49	29,18		S	1	Rain.
16	8	0	38	48	29,59		W	1	Fine.
	3	0	38	50	29,73		W	1	Cloudy.

METEOROLOGICAL JOURNAL
for January, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Jan. 17	8	0	38	48	29,48	Rain this Month 0,733 Inches.	W	1	Cloudy.
	3	0	38	51	29,52		W	1	Fine.
18	8	0	46	47	29,70		W	1	Fine.
	3	0	45	50	29,68		W	1	Fine.
19	8	0	35	47	29,68		SE	1	Fine.
	3	0	41	50	29,68		S	1	Cloudy.
20	8	0	36	47	29,50		W	1	Cloudy.
	3	0	47	50	29,47		SW	1	Fair.
21	8	0	36	46	29,23		E	1	Rain.
	3	0	42	47	29,22		SSE	1	Cloudy.
22	8	0	39	47	29,38		SE	1	Rain.
	3	0	43	48	29,43		ESE	1	Fair.
23	8	0	39	47	29,45		E	1	Cloudy and thick.
	3	0	41	52	29,46		E	1	Cloudy.
24	8	0	37	48	29,21		E	1	Cloudy.
	3	0	38	49	29,55		E	1	Cloudy.
25	8	0	37	47	29,02		E	1	Cloudy.
	3	0	38	51	29,05		N	1	Cloudy.
26	8	0	36	48	29,22		N	1	Cloudy.
	3	0	38	51	29,33		N	1	Cloudy.
27	8	0	38	48	29,55		N	1	Cloudy.
	3	0	40	51	29,74		NE	1	Cloudy.
28	8	0	33	46	30,06		NE	1	Cloudy and hazy.
	3	0	36	46	30,14		N	1	Cloudy.
29	8	0	31	44	30,33		N by E	1	Cloudy.
	3	0	32	48	30,35		E	1	Cloudy.
30	8	0	28	43	30,41		SE	1	Fair.
	3	0	32	48	30,42		SE	1	Fine.
31	8	0	25	41	30,33		S by E	1	Fair.
	3	0	34	46	30,21		S	1	Cloudy.

METEOROLOGICAL JOURNAL
for February, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Feb. 1	8	0	27	42	29,95	Rain this Month 1,675 Inches.	SSE	1	Cloudy.
	3	0	36	47	29,89		S	1	Fine.
2	8	0	32	44	29,68		SSE	1	Thick and foggy.
	3	0	36	47	29,59		E	1	Cloudy.
3	8	0	42	45	29,47		W	1	Cloudy.
	3	0	45	50	29,50		S	1	Rain.
4	8	0	40	47	29,48		S	1	Rain.
	3	0	45	47	29,43		W	1	Rain.
5	8	0	35	45	29,43		W	1	Foggy.
	3	0	38	49	29,44		W	1	Cloudy.
6	8	0	38	47	29,13		E	1	Rain.
	3	0	39	48	29,02		E by N	1	Rain.
7	8	0	33	45	28,88		N	1	Snow, a fall of snow in the night.
	3	0	31	47	29,02		NE	1	Fine.
8	8	0	22	42	29,31		N	1	Fine.
	3	0	28	48	29,48		N	1	Fine.
9	8	0	22	41	29,64		E	1	Foggy.
	3	0	27	47	29,65		E	1	Fine.
10	8	0	19	39	29,69		S	1	Foggy.
	3	0	31	43	29,70		S	1	Cloudy.
11	8	0	30	40	29,81		W	1	Sleet.
	3	0	35	40	29,87		W	1	Fair.
12	8	0	26	37	30,27		N	1	Fine.
	3	0	35	45	30,34		N	1	Fine.
13	8	0	27	39	30,35		NW	1	Fine
	3	0	38	47	30,32		W	1	Fine.
14	8	0	32	42	30,34		W	1	Fine.
	3	0	41	49	30,38		W	1	Fine.

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for February, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Feb. 15	8	0	37	42	30,37	Rain this Month 1,675 Inches.	NW	1	Foggy.
	3	0	42	48	30,29		W	1	Fine.
16	8	0	40	46	30,01		NW	1	Cloudy.
	3	0	46	51	29,88		W	1	Cloudy.
17	8	0	37	47	29,88		NW	1	Fine.
	3	0	40	51	29,91		N	1	Fine.
18	8	0	30	44	30,09		N	1	Cloudy.
	3	0	32	44	29,92		W	1	Cloudy.
19	8	0	40	44	29,92		NW	1	Foggy.
	3	0	44	50	29,98		W	1	Cloudy.
20	8	0	42	47	30,03		W	1	Fine.
	3	0	45	50	29,88		W	2	Cloudy.
21	8	0	41	49	30,08		W	1	Cloudy.
	3	0	47	55	30,11		W	1	Fine.
22	8	0	44	50	30,12		S	1	Cloudy.
	3	0	47	54	30,15		W	1	Cloudy.
23	8	0	42	52	30,26		SW	1	Cloudy
	3	0	50	57	30,27		S	1	Fine.
24	8	0	40	51	30,19		S	1	Fine.
	3	0	55	59	30,15		S	1,2	Fair.
25	8	0	47	53	30,03		W	1	Cloudy.
	3	0	50	54	29,95		W	1	Rain.
26	8	0	39	51	30,11		NW	1	Fine.
	3	0	50	56	30,15		NW	1	Fine.
27	8	0	36	50	29,84		W	1	Rain.
	3	0	48	55	29,62		W	1	Cloudy.
28	8	0	37	51	29,77		N	1	Fair.
	3	0	45	56	29,87		N	1	Fair.
29	8	0	30	49	29,94		N	1	Hazy.
	3	0	32	50	29,91		N	1	Fine.

METEOROLOGICAL JOURNAL

for March, 1816.

1816	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
Mar. 1	8	0	32	49	29,90		W	1	Cloudy.
	3	0	44	56	29,91		SW	1	Fine.
2	8	0	37	48	29,59		S	1.2	Cloudy.
	3	0	40	53	29,38		SSE	1	Rain.
3	8	0	38	49	29,25		W	1	Cloudy.
	3	0	40	49	29,24		SSE	1	Cloudy.
4	8	0	36	47	29,24		W	1	Fine.
	3	0	39	50	29,19		W	1.2	Rain.
5	8	0	34	47	29,20		W	1	Fine.
	3	0	45	53	29,23		W	1	Cloudy.
6	8	0	40	49	29,02		W	1	Cloudy.
	3	0	43	57	28,99	Rain this Month 0,425 Inches.	SW	1	Rain.
7	8	0	41	49	29,13		W	1	Cloudy.
	3	0	46	55	29,20		W	1	Cloudy.
8	8	0	39	50	29,12		S	1	Rain.
	3	0	40	53	29,02		S	1	Rain.
9	8	0	36	50	29,34		S	1	Cloudy.
	3	0	39	52	29,50		E	1	Cloudy.
10	8	0	35	48	29,71		NNW	1	Fair.
	3	0	44	51	29,83		N	1	Fine.
11	8	0	41	47	29,93		SW	1	Cloudy.
	3	0	46	52	29,82		W	1	Rain.
12	8	0	47	51	29,70		SW	1	Rain.
	3	0	48	55	29,66		W	1	Cloudy.
13	8	0	45	59	29,72		W	1	Fine.
	3	0	53	59	29,84		W	1	Fair.
14	8	0	43	53	29,91		E	1	Rain.
	3	0	52	58	29,70		S	1.2	Cloudy.
15	8	0	46	54	29,38		SW	2	Cloudy.
	3	0	43	56	29,49		W	1.2	Rain.
16	8	0	36	52	29,84		S by E	1	Fine.
	3	0	45	55	29,75		E	1	Cloudy.

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for March, 1816.

1816	Time.		Therm. without.	Therm. within.	Barom.		Wind.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Mar. 17	8	0	37	51	29,80	Rain this Month 0,425 Inches.	W	1	Cloudy.
	3	0	43	52	29,73		W	1,2	Cloudy.
18	8	0	40	50	29,57		W	1	Rain.
	3	0	49	55	29,56		N	1	Cloudy.
19	8	0	41	50	29,61		W	1	Cloudy.
	3	0	47	55	29,65		NW	1	Cloudy.
20	8	0	40	50	29,95		NE	1	Cloudy.
	3	0	47	55	29,98		N	1	Fine.
21	8	0	37	48	30.05		E	1	Cloudy.
	3	0	52	57	30,05		W	1	Cloudy.
22	8	0	40	58	30,09		SSE	1	Cloudy.
	3	0	52	52	30,10		S	1	Cloudy.
23	8	0	40	57	30,24		E	1	Cloudy.
	3	0	47	51	30,29		E	1	Cloudy.
24	8	0	39	49	30,34		E	1	Cloudy.
	3	0	39	52	30,30		E	1	Cloudy.
25	8	0	39	49	30,14		E	1	Cloudy.
	3	0	41	52	30.09		E	1	Cloudy.
26	8	0	40	49	30,15		E	1	Cloudy.
	3	0	42	52	30,15		E	1	Cloudy.
27	8	0	39	49	30,19		E	1	Cloudy.
	3	0	40	52	30,19		E	1	Cloudy.
28	8	0	36	47	30,14		E	1	Fine.
	3	0	48	57	30,14		SSE	1	Fine.
29	8	0	37	49	30,16		SE	1	Cloudy.
	3	0	42	53	30,16		E	1	Cloudy.
30	8	0	38	49	30,23		E	1	Cloudy.
	3	0	40	52	30,23		SE	1	Fine.
31	8	0	41	48	30,23		SE	1	Fine.
	3	0	43	53	30,21		SE	1	Fine.

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for April, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Apr. 1	8	0	38	47	30,11	Rain this Month 1,020 Inches.	SE	1	Fine.
	3	0	45	54	30,00		SE	1	Fine.
2	8	0	38	48	29,86		SE	1	Fine.
	3	0	45	56	29,83		SE	1	Fine.
3	8	0	38	48	29,82		E	1	Fine.
	3	0	45	54	29,88		SE	1	Fine.
4	8	0	41	48	29,98		SE	1	Fine.
	3	0	46	57	29,99		SE	1	Fine.
5	8	0	35	49	30,00		NE	1	Cloudy.
	3	0	48	57	29,88		SSE	1	Fine.
6	8	0	42	51	29,66		N	1	Thick and cloudy.
	3	0	57	58	29,55		NW	2	Cloudy.
7	8	0	42	51	29,21		W	1	Cloudy.
	3	0	44	53	29,10		NW	1	Cloudy.
8	8	c	39	50	29,07		NW	1	Cloudy.
	3	0	45	52	29,12		N NW	1	Cloudy.
9	8	0	39	50	29,20		N	1	Cloudy.
	3	0	43	53	29,19		NE	1	Rain.
10	8	0	43	50	29,16		S	1	Cloudy.
	3	0	50	55	29,29		E	1	Cloudy.
11	8	0	43	52	29,53		N	1	Rain.
	3	0	47	56	29,51		SE	1	Rain.
12	8	0	41	52	29,69		W	1	Cloudy.
	3	0	46	53	29,69		W	1	Cloudy.
13	8	0	38	50	29,76		NE	1	Cloudy.
	3	0	41	53	29,77		NE	1	Cloudy.
14	8	0	34	48	29,55		N	1	Cloudy.
	3	0	39	47	29,55		N	1	Cloudy.
15	8	0	36	45	29,64		NW	1	Cloudy.
	3	0	46	51	29,66		W	1	Cloudy.
16	8	0	38	47	29,62		SW	1	Fine.
	3	0	47	53	29,52		SW	1	Cloudy.

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for April, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Apr. 17	8	0	43	49	29,44	Rain this Month 1,020 Inches.	SW	1	Rain.
	3	0	53	54	29,48		N	1	Cloudy.
18	8	0	43	50	29,58		SW	1	Fine.
	3	0	52	56	29,58		SW	1	Fine.
19	8	0	43	52	29,66		NW	1	Fine.
	3	0	54	58	29,88		WNW	1	Cloudy.
20	8	0	44	52	30,11		S	1	Fine.
	3	0	52	59	30,09		E	1	Fine.
21	8	0	43	46	29,78		E	1	Cloudy.
	3	0	48	54	29,72		E	1	Cloudy.
22	8	0	48	52	29,70		NE	1	Rain.
	3	0	54	56	29,71		E	1	Rain.
23	8	0	53	55	29,71		E	1	Cloudy.
	3	0	61	62	29,73		E	1	Fine.
24	8	0	52	55	29,81		N	1	Fine.
	3	0	59	65	29,83		E	1	Fine.
25	8	0	55	58	29,94		E	1	Fair.
	3	0	62	66	29,98		E	1	Fine.
26	8	0	50	60	30,09		N	1	Fine.
	3	0	60	66	30,09		E	1	Fine.
27	8	0	50	60	30,04		N	1	Fine.
	3	0	60	67	29,98		E	1	Fine.
28	8	0	54	59	29,92		E	1	Fine.
	3	0	64	62	29,82		E	1	Fine.
29	8	0	55	60	29,61		W	1	Cloudy.
	3	0	59	61	29,58		S	1	Cloudy.
30	8	0	52	58	29,53		E	1	Cloudy.
	3	0	58	60	29,53		SW	1	Cloudy.

METEOROLOGICAL JOURNAL
for May, 1816.

1816	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
May 1	8	0	50	58	29,65	Rain this Month 0,902 Inches.	W	1	Fine.
	3	0	54	56	29,66		SW	1	Cloudy.
2	8	0	53	57	29,73		W	1	Cloudy.
	3	0	56	60	29,81		W	1	Fine.
3	8	0	48	57	29,99		W	1	Cloudy.
	3	0	53	60	29,99		W	1	Rain.
4	8	0	49	57	30,06		W	1	Cloudy and hazy.
	3	0	50	59	30,10		W	1	Rain.
5	8	0	48	56	29,90		S	1.2	Rain.
	3	0	55	57	29,75		W	1	Cloudy.
6	8	0	50	55	29,85		NW	1	Cloudy.
	3	0	55	58	29,96		N	1	Cloudy.
7	8	0	49	56	29,96		W	1	Fair.
	3	0	51	60	29,85		SW	1	Cloudy.
8	8	0	50	56	29,64		W	1	Cloudy.
	3	0	50	59	29,47		S	1	Rain.
9	8	0	47	55	29,48		W	1	Rain.
	3	0	53	60	29,58		N	1.2	Rain.
10	8	0	46	56	29,55		S	1	Cloudy.
	3	0	51	61	29,21		NW	1.2	Rain.
11	8	0	42	54	29,18		N	1	Cloudy.
	3	0	49	57	29,37		N	1.2	Cloudy.
12	8	0	41	54	29,33		N	1	Cloudy.
	3	0	46	57	29,40		NW	1	Cloudy.
13	8	0	41	51	29,54		WNW	1	Cloudy.
	3	0	50	57	29,60		NNE	1	Cloudy.
14	8	0	45	53	29,83		W	1	Cloudy.
	3	0	46	56	29,84		SW	1	Cloudy.
15	8	0	48	54	29,86		E	1	Rain.
	3	0	54	58	29,86		SW	1	Cloudy.
16	8	0	51	56	29,88		E	1	Fair.
	3	0	63	61	29,81		SE	1	Fine.

METEOROLOGICAL JOURNAL
for May, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain	Winds. Points.	Winds. Str.	Weather.
May 17	8	0	57	59	29,75	Rain this Month 0,902 Inches.	E	1	Fine.
	3	0	63	64	29,74		E	1	Fine.
18	8	0	48	58	29,78		Var.	1	Fine.
	3	0	53	58	29,80		E	1	Cloudy.
19	8	0	52	56	29,84		W	1	Cloudy.
	3	0	54	57	29,84		N	1	Cloudy.
20	8	0	48	55	29,90		E	1	Cloudy.
	3	0	64	64	29,91		E	1	Fine.
21	8	0	52	58	29,95		E	1	Fine.
	3	0	65	65	29,91		E	1	Fine.
22	8	0	53	58	29,86		NE	1	Cloudy.
	3	0	64	64	29,85		N	1	Fine.
23	8	0	53	57	29,90		N	1	Cloudy.
	3	0	63	64	29,91		NE	1	Fine.
24	8	0	52	60	29,87		N	1	Cloudy.
	3	0	57	63	29,84		N	1	Cloudy.
25	8	0	53	60	29,82		W	1	Cloudy.
	3	0	60	62	29,84		SW	1	Cloudy.
26	8	0	53	58	30,11		WNW	1	Cloudy.
	3	0	60	62	30,14		SW	1	Fine.
27	8	0	48	57	29,97		E	1	Rain.
	3	0	55	59	30,08		E	1	Cloudy.
28	8	0	53	58	30,14		S	1	Cloudy.
	3	0	61	61	30,15		E	1	Fine.
29	8	0	55	59	30,10		S	1	Cloudy.
	3	0	62	63	30,01		W	1	Cloudy.
30	8	0	56	60	29,89		W	1	Cloudy.
	3	0	66	64	29,88		W	1	Fine.
31	8	0	56	60	29,85		N	1	Cloudy.
	3	0	62	64	29,86		NNW	1	Cloudy.

METEOROLOGICAL JOURNAL

for June, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
June 1	8	0	55	61	30,01	Rain this Month 0,931 Inches.	NNW	1	Fine.
	3	0	66	69	30,03		W	1	Fine.
2	8	0	61	64	30,01		W	1	Fine.
	3	0	70	68	29,96		E	1	Fine.
3	8	0	55	63	30,01		W	1	Cloudy and hazy.
	3	0	63	68	30,02		NW	1	Fine.
4	8	0	55	62	30,07		W	1	Cloudy.
	3	0	65	65	29,95		N	1	Cloudy.
5	8	0	56	61	29 92		N	1	Cloudy.
	3	0	60	61	29,76		N	1	Cloudy.
6	8	0	49	53	29,79		N	1	Cloudy.
	3	0	51	57	29,86		W	1	Cloudy.
7	8	0	48	56	29,83		N	1	Cloudy.
	3	0	59	58	29,66		N	1	Cloudy.
8	8	0	53	57	29,48		NE	1	Rain.
	3	0	53	56	29,34		W	1	Rain.
9	8	0	50	56	29,24		W	1	Rain.
	3	0	54	57	29,35		N	1	Cloudy.
10	8	0	49	54	29,57		N	1	Cloudy.
	3	0	53	58	29,72		N	1	Cloudy.
11	8	0	52	56	29,95		N	1	Fine.
	3	0	59	60	29,98		N	1	Fine.
12	8	0	54	57	30,10		S	1	Fine.
	3	0	62	63	30,10		S	1	Fine.
13	8	0	57	58	30,08		S	1	Fine.
	3	0	61	63	30,02		W	1	Cloudy.
14	8	0	59	60	30,01		N	1	Cloudy.
	3	0	57	61	30,01		NNW	1	Rain.
15	8	0	52	59	29,95		N	1	Cloudy.
	3	0	61	60	29,94		NE	1	Cloudy.
16	8	0	53	58	30,00		N	1	Cloudy.
	3	0	59	58	30,01		N	1	Cloudy.

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for June, 1816.

1816	Time.		Therm. without.	Therm. within.	Barom.				Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
June 17	8	0	52	57	30,01	Rain this Month 0,931 Inches.	SW	1	Cloudy.
	3	0	62	59	29,98		N	1	Cloudy.
18	8	0	53	57	29,93		W	1	Cloudy.
	3	0	64	62	29,91		W	1	Cloudy.
19	8	0	55	59	29,95		W	1	Cloudy.
	3	0	65	64	29,99		W	1	Cloudy.
20	8	0	57	59	30,06		W	1	Cloudy.
	3	0	65	62	30,06		N	1	Cloudy.
21	8	0	59	61	30,07		SE	1	Cloudy.
	3	0	67	62	30,06		E	1	Cloudy.
22	8	0	55	61	30,01		E	1	Cloudy.
	3	0	64	63	30,03		E	1	Fine.
23	8	0	58	62	29,89		SW	1	Cloudy.
	3	0	60	62	29,77		W	1	Rain.
24	8	0	55	60	29,79		W	1	Cloudy.
	3	0	65	62	29,83		W	1	Cloudy.
25	8	0	56	60	29,94		N	1	Cloudy.
	3	0	67	66	29,92		NE	1	Cloudy.
26	8	0	60	62	29,82		E	1	Cloudy.
	3	0	59	62	29,71		ESE	1	Rain.
27	8	0	56	61	29,53		N	1	Rain.
	3	0	61	63	29,71		E	1	Cloudy.
28	8	0	55	61	29,97		N	1	Cloudy.
	3	0	62	64	30,03		NE	1	Cloudy.
29	8	0	57	62	30,05		N	1	Cloudy.
	3	0	67	67	30,02		S	1	Cloudy.
30	8	0	57	61	29,87		WSW	1	Cloudy.
	3	0	66	65	29,80		S	1,2	Fine.

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for July, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
July 1	8	0	56	62	29,67	Rain this Month 2,789 Inches.	WNW	1	Rain.
	3	0	61	63	29,67		N	1	Cloudy.
2	8	0	56	61	29,75		SW	1	Fine.
	3	0	64	66	29,76		SW	1	Fine.
3	8	0	57	61	29,77		W	1	Cloudy.
	3	0	64	63	29,78		NW	1	Cloudy.
4	8	0	56	61	29,81		W	1	Cloudy.
	3	0	57	62	29,73		S	1	Cloudy.
5	8	0	55	60	29,65		N	1	Cloudy.
	3	0	61	64	29,73		NNW	1	Cloudy. Thunder storm at 2 P.M.
6	8	0	56	60	29,82		S	1	Cloudy.
	3	0	53	59	29,76		N	1	Fine.
7	8	0	60	60	29,65		E	1	Cloudy.
	3	0	59	61	29,61		N	1	Fine.
8	8	0	58	56	29,61		E	1	Cloudy.
	3	0	63	62	29,62		E	1	Fine.
9	8	0	57	60	29,60		SE by S	1	Fine.
	3	0	63	62	29,62		SE	2	Rain.
10	8	0	56	60	29,55		S	1	Cloudy.
	3	0	63	64	29,53		S	2	Fine.
11	8	0	56	60	29,55		W	1	Fine.
	3	0	63	63	29,57		W	1	Rain.
12	8	0	56	61	29,67		W	1	Cloudy.
	3	0	61	62	29,75		N	1	Cloudy.
13	8	0	56	60	29,88		NW	1	Cloudy.
	3	0	64	64	29,88		NW	1	Fine.
14	8	0	58	61	29,88		S	1	Cloudy.
	3	0	59	61	29,74		S	1	Rain.
15	8	0	58	61	29,61		W	1	Rain.
	3	0	65	64	29,60		WNW	1	Cloudy.
16	8	0	56	61	29,61		E	1	Rain.
	3	0	58	62	29,61		SW	1	Rain.

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1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
July 17	8	0	57	61	29,56	Rain this Month 2,789 Inches.	NNW	1	Cloudy.
	3	0	59	61	29,49		S	1	Cloudy.
18	8	0	56	59	29,33		SW	2	Fine.
	3	0	62	62	29,37		SW	1,2	Fair.
19	8	0	53	59	29,44		S	1,2	Rain.
	3	0	59	61	29,57		NW	2	Rain.
20	8	0	63	61	29,68		S	1	Fine.
	3	0	69	70	29,66		E	1	Fine.
21	8	0	65	65	29,46		SSE	1	Cloudy.
	3	0	58	64	29,53		S	2,3	Cloudy.
22	8	0	58	62	29,71		W	2	Cloudy.
	3	0	64	65	29,74		WSW	1	Fine.
23	8	0	57	62	29,58		S	1	Cloudy.
	3	0	64	66	29,54		S	1	Cloudy.
24	8	0	57	61	29,55		W	1	Cloudy.
	3	0	62	64	29,56		SW	1	Cloudy.
25	8	0	57	61	29,55		W	1	Cloudy.
	3	0	60	62	29,60		W	1	Cloudy.
26	8	0	57	61	29,81		SE	1	Cloudy.
	3	0	60	61	29,87		W	1	Cloudy.
27	8	0	58	61	29,92		W	1	Cloudy.
	3	0	62	63	29,88		W	1	Cloudy.
28	8	0	55	61	29,77		NW	1	Cloudy.
	3	0	62	64	29,72		N	1	Fine.
29	8	0	53	60	29,67		S	1	Cloudy dull weather.
	3	0	58	62	29,58		W	1	Cloudy.
30	8	0	54	58	29,61		N	1	Cloudy.
	3	0	56	60	29,45		SW	1	Fair.
31	8	0	52	56	29,43		W	1	Fair.
	3	0	53	58	29,35		N	1	Fine.

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1816	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
Aug. 1	7	0	56	60	29,53	Rain this Month 1,176 Inches.	NW	1	Fine.
	3	0	59	62	29,66		N	1	Cloudy.
2	7	0	58	59	29,77		E	1	Fine.
	3	0	60	62	29,77		W	1	Cloudy.
3	7	0	58	60	29,78		NW	1	Fair.
	3	0	61	57	29,85		W	1	Fine.
4	7	0	59	60	29,82		NE	1	Fine.
	3	0	60	58	29,76		NE b E	1	Cloudy.
5	7	0	55	58	29,77		NE	1	Hazy.
	3	0	61	60	29,77		NW	1	Cloudy.
6	7	0	53	58	29,88		E	1	Fine.
	3	0	60	63	29,84		NE	1	Rain.
7	7	0	59	60	29,86		E	1	Cloudy.
	3	0	61	65	29,88		N	1	Cloudy,
8	7	0	60	62	29,73		W	1	Fair, some rain in the night.
	3	0	59	65	29,72		W	1	Fine.
9	7	0	61	67	29,73		E	1	Fair.
	3	0	60	63	29,75		N	1	Cloudy.
10	7	0	63	69	29,76		E	1	Cloudy.
	3	0	58	61	29,77		N	1	Cloudy.
11	7	0	64	67	30,02		N	1	Rain.
	3	0	66	64	30,01		N	1	Fine.
12	7	0	62	63	30,01		N	1	Rain.
	3	0	59	62	30,10		E	1	Fair.
13	7	0	65	68	30,11		W	1	Fine.
	3	0	62	69	30,03		W	1	Fine.
14	7	0	67	67	30,02		E	1,2	Fine.
	3	0	64	65	30,11		N	1	Fair.
15	7	0	69	69	30,01		E	1	Fine.
	3	0	58	63	29,43		N	1	Fair.
16	7	0	64	66	29,51		N	1	Fine.
	3	0	62	64	29,52		N	1	Fine.

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1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Aug. 17	7	0	67	64	29,61	Rain this Month 1,176 Inches.	E	1	Fair.
	3	0	62	63	29,62		S	1	Rain.
18	7	0	58	60	29,94		SE	1	Cloudy and hazy.
	3	0	60	65	29,92		NNE	1	Cloudy.
19	7	0	61	60	29,94		ENE	1	Fair.
	3	0	62	63	29,83		E	1	Fine.
20	7	0	62	61	29,77		N	1	Fine.
	3	0	59	64	29,79		W	1	Cloudy.
21	7	0	63	60	29,76		SSW	1	Hazy.
	3	0	63	64	30,02		E	1	Fine.
22	7	0	59	61	29,96		N	1	Fine.
	3	0	61	65	29,99		N	1	Fine.
23	7	0	62	65	29,97		E	1	Fair.
	3	0	63	62	30,09		E	1	Fair.
24	7	0	59	57	30,13		N	1	Cloudy.
	3	0	69	61	30,21		E	1	Fine.
25	7	0	56	50	30,19		N	1	Fair.
	3	0	61	52	30,22		W	1	Showers.
26	7	0	61	52	30,24		NE	1	Fair.
	3	0	64	55	30,21		N	1,2	Rain.
27	7	0	60	57	29,99		S	1	Rain.
	3	0	68	59	29,87		SE	1	Fine.
28	7	0	60	60	29,99		NW	1	Cloudy.
	3	0	62	52	29,99		E	1	Cloudy.
29	7	0	67	58	30,11		SE	1	Cloudy.
	3	0	58	50	30,01		SSW	1	Fair.
30	7	0	59	56	30,09		W	1	Fine.
	3	0	59	54	30,08		E	1	Fine.
31	7	0	58	59	30,09		N	1	Cloudy.
	3	0	57	61	30,22		E	1	Cloudy.

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1816	Time. H.	M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Str.	Weather.
Sep. 1	7	0	54	59	29,87	Rain this Month 0,824 Inches.	E	1	Fair.
	3	0	58	61	29,89		N	1	Cloudy.
2	7	0	54	56	29,97		S	1	Cloudy.
	3	0	58	60	30,00		E	1,2	Rain.
3	7	0	56	61	30,11		N	1	Fair.
	3	0	59	63	30,12		NE	1	Cloudy.
4	7	0	60	62	30,11		W	1	Fair.
	3	0	62	65	30,16		S	1	Rain.
5	7	0	64	67	30,07		SW	1	Rain.
	3	0	69	67	30,04		N	1	Fair.
6	7	0	64	60	30,08		NE	1	Fine.
	3	0	67	64	30,09		NW	1	Rain.
7	7	0	67	61	30,21		W	1	Cloudy.
	3	0	62	59	30,11		E	1	Cloudy.
8	7	0	69	64	30,19		N	1	Cloudy.
	3	0	67	60	29,97		NW	1	Cloudy.
9	7	0	58	62	29,82		N	1	Cloudy, rain in the night.
	3	0	59	68	29,91		S	1	Fair.
10	7	0	61	59	29,85		SE	1	Rain.
	3	0	62	67	29,89		S	1	Rain.
11	7	0	64	58	29,99		N	1	Rain.
	3	0	66	62	30,01		E	1	Cloudy.
12	7	0	66	61	30,11		N	1	Rain.
	3	0	63	64	30,11		S	1	Cloudy.
13	7	0	64	59	29,87		N	1	Cloudy.
	3	0	62	58	29,89		E	1	Cloudy.
14	7	0	66	57	29,91		E	1	Cloudy.
	3	0	64	60	29,97		SW	1	Fair.
15	7	0	67	62	29,88		SSE	1	Cloudy.
	3	0	65	63	29,89		N	1	Cloudy.
16	7	0	62	64	29,92		NE	1	Rain.
	3	0	67	67	29,99		NNE	1	Rain.

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1816	Time.		Therm. without.	Therm. within.	Barom.		Winds.		Weather.
	H.	M.	°	°	Inches.		Points.	Str.	
Sep. 17	7	0	65	67	29,85	Rain this Month 0,824 Inches.	N	1	Fair.
	3	0	69	62	29,75		NW	1	Fine.
18	7	0	67	64	29,73		SW	1	Cloudy.
	3	0	62	63	29,77		SSW	1	Cloudy.
19	7	0	69	61	29,83		E	1	Cloudy.
	3	0	58	69	29,97		W	1	Fair.
20	7	0	63	59	29,99		W by N	1	Cloudy.
	3	0	56	61	30,01		NE	1	Fair.
21	7	0	59	58	29,99		N	1	Fair.
	3	0	60	63	29,88		N	1	Fine.
22	7	0	60	62	29,81		E	1	Fine.
	3	0	58	60	29,74		W	1	Fine.
23	7	0	62	59	29,82		E	1	Fine.
	3	0	57	58	29,85		N	1	Cloudy.
24	7	0	58	61	29,99		NW	1	Cloudy.
	3	0	60	62	29,94		W	1	Cloudy.
25	7	0	59	60	30,07		E	1	Fair.
	3	0	62	61	30,08		N	1	Cloudy.
26	7	0	56	61	29,99		E	1	Cloudy.
	3	0	58	63	30,11		NE	1	Cloudy.
27	7	0	57	59	30,18		N	1	Fair.
	3	0	56	65	30,11		E	1	Cloudy.
28	7	0	55	62	29,97		N	1	Cloudy.
	3	0	54	62	29,98		W	1	Rain.
29	7	0	53	58	29,81		N	1	Rain.
	3	0	62	61	29,76		E	1	Fair.
30	7	0	57	60	29,77		E	1	Cloudy.
	3	0	60	57	29,81		NW	1	Cloudy.

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1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain this Month 0,888 inches.	Winds. Points.	Winds. Str.	Weather.
Oct. 1	7	0	57	61	29,75		N	1	Cloudy.
	3	0	59	60	29,61		E	1	Cloudy.
2	7	0	55	57	29,58		W	1	Rain.
	3	0	59	60	29,59		NW	1	Rain.
3	7	0	57	56	29,78		N	1	Fair.
	3	0	54	58	29,93		S	1	Cloudy.
4	7	0	55	56	29,88		N	1	Cloudy.
	3	0	59	58	29,86		SW	1	Cloudy.
5	7	0	56	58	29,91		SE	1	Cloudy.
	3	0	61	61	29,86		SE	1	Cloudy.
6	7	0	59	59	29,84		W	1	Fine.
	3	0	61	61	29,86		SE	1	Rain.
7	7	0	58	60	29,79		E	1	Rain.
	3	0	58	61	29,83		E	1	Rain.
8	7	0	57	60	29,93		WNW	1	Cloudy.
	3	0	59	63	29,99		E	1	Fine.
9	7	0	56	61	30,04		E	1	Cloudy.
	3	0	58	61	30,04		E	1	Cloudy.
10	7	0	56	60	30,06		E	1	Cloudy.
	3	0	59	64	30,08		E	1	Cloudy.
11	7	0	55	60	30,00		NW	1	Cloudy.
	3	0	60	62	30,00		N	1	Rain.
12	7	0	51	59	30,1[illegible]		E	1	Cloudy.
	3	0	55	60	30,14		W	1	Cloudy.
13	7	0	52	59	30,13		SW	1	Cloudy.
	3	0	54	58	30,08		W	1	Cloudy.
14	7	0	48	57	30,11		W	1	Cloudy.
	3	0	58	61	30,11		W	1	Fine.
15	7	0	46	57	30,20		W	1	Cloudy and hazy.
	3	0	59	60	30,15		SW	1	Cloudy.
16	7	0	48	57	30,08		E	1	Foggy.
	3	0	55	62	29 96		W	1	Fine.

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1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Oct. 17	7	0	49	57	29,79	Rain this Month 0,888 Inches.	E	1	Rain.
	3	0	53	57	29,79		N	1	Rain.
18	7	0	47	55	29,76		NW	1	Cloudy.
	3	0	52	58	29,83		NW	1	Fine.
19	7	0	42	52	29,95		W	1	Cloudy.
	3	0	54	55	29,83		W	1,2	Cloudy.
20	7	0	46	54	29,62		W	1	Fine.
	3	0	52	55	29,60		NW	1	Cloudy.
21	7	0	42	51	29,63		N	1	Fine.
	3	0	50	52	29,70		NW	1	Cloudy.
22	7	0	45	52	29,68		W	1	Rain.
	3	0	53	55	29,71		NW	1	Cloudy.
23	7	0	37	50	29,94		W	1	Fine.
	3	0	52	52	29,90		SW	1	Fine.
24	7	0	46	51	29,83		S	r	Cloudy.
	3	0	49	52	29,69		E	r	Cloudy.
25	7	0	47	51	29,48		E	1	Cloudy.
	3	0	46	52	29,33		S	1,2	Rain.
26	7	0	42	50	29,59		SE	1	Fine.
	3	0	49	55	29,63		E	1	Fine.
27	7	0	50	52	29,62		E	1	Fine.
	3	0	52	54	29,63		E	1	Cloudy.
28	7	0	51	52	29,60		E	1	Cloudy.
	3	0	55	54	29,60		E	1	Cloudy.
29	7	0	49	53	29,60		E	1	Cloudy.
	3	0	57	55	29,54		E	1	Cloudy.
30	7	0	49	54	29,21		E	1	Rain.
	3	0	54	56	29,17		E	1	Cloudy.
31	7	0	45	54	29,19		SSW	1	Fine.
	3	0	52	53	29,22		S	1	Cloudy.

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1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Nov. 1	8	0	45	54	29,49	Rain this Month 2,120 Inches.	W	1	Cloudy.
	3	0	52	59	29,82		W	1	Fine.
2	8	0	42	54	29,39		E	1	Foggy.
	3	0	48	57	29,30		SE	1	Rain.
3	8	0	44	54	29,34		S	1	Fine.
	3	0	51	57	29,51		S	1	Fine.
4	8	0	45	53	29,66		N	1	Rain.
	3	0	51	58	29,67		NE	1	Cloudy.
5	8	0	48	55	29,69		N	1	Rain.
	3	0	49	57	29,65		E	1	Rain.
6	8	0	46	55	29,44		SE	1	Cloudy.
	3	0	50	58	29,40		W	1	Rain.
7	8	0	39	53	29,37		W	1	Fine.
	3	0	42	57	29,36		W	1	Fine.
8	8	0	32	51	29,51		E	1	Foggy.
	3	0	40	55	29,54		SW	1	Fine.
9	8	0	43	58	28,94		W	1	Fine, violent storm of rain in [the night.
	3	0	46	57	28,83		S	1	Cloudy.
10	8	0	37	52	29,32		N	1	Snow.
	3	0	41	53	29,49		W	1	Fine.
11	8	0	29	46	29,76		W	1	Fine.
	3	0	39	52	29,70		W	1	Cloudy.
12	8	0	43	49	29,25		NW	2	Rain.
	3	0	46	54	29,61		NW	1	Fine.
13	8	0	50	52	29,74		W	1	Cloudy.
	3	0	53	56	29,75		W	1	Cloudy.
14	8	0	43	51	29,71		W	1	Cloudy.
	3	0	45	55	29,61		W	1	Fine.
15	8	0	32	50	29,51		W	1	Fine.
	3	0	36	55	29,57		W	1	Fine.
16	8	0	35	49	29,79		N	1	Cloudy.
	3	0	39	53	29,80		N	1	Cloudy.

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1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Winds. Points.	Winds. Str.	Weather.
Nov. 17	8	0	33	48	30,07	N	1	Fine.
	3	0	37	49	30,07	W	1	Fine.
18	8	0	43	47	29,76	SW	1	Rain.
	3	0	46	52	29,66	W	1	Rain.
19	8	0	39	50	29,74	S	1	Fair.
	3	0	46	51	29,84	S	1	Cloudy.
20	8	0	47	50	29,98	S	1	Fine.
	3	0	49	54	30,01	SE	1,2	Cloudy.
21	8	0	42	51	30,05	E	1	Hazy.
	3	0	43	54	29,97	E	1,2	Cloudy.
22	8	0	37	54	29,84	E	1	Cloudy.
	3	0	42	52	29,80	E	1	Cloudy.
23	8	0	33	47	29,77	E	1	Cloudy.
	3	0	40	50	29,91	E	1	Cloudy.
24	8	0	26	46	29,94	W	1	Thick and cloudy.
	3	0	33	45	29,94	W	1	Fine.
25	8	0	33	43	29,95	S	1	Cloudy.
	3	0	40	46	29,96	S	1	Cloudy.
26	8	0	43	47	29,91	E	1	Thick and foggy.
	3	0	43	50	29,96	N	1	Cloudy.
27	8	0	34	45	29,25	W	1	Fine.
	3	0	46	51	29,28	W	1	Cloudy.
28	8	0	43	49	29,33	W	1	Cloudy.
	3	0	44	54	29,26	W	1	Cloudy.
29	8	0	36	47	29,46	W	1	Thick and foggy.
	3	0	44	54	29,48	N	1	Fine.
30	8	0	34	47	29,49	N	1	Fine.
	3	0	42	52	30,63	N	1	Cloudy.

Rain this Month 2,120 Inches.

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1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.	Rain	Winds. Points.	Winds. Str.	Weather.
Dec. 1	8	0	38	47	30,64	Rain this Month 1,741 Inches.	N	1	Cloudy.
	3	0	38	48	30,61		W	1	Cloudy.
2	8	0	34	45	30,45		WNW	1	Cloudy and hazy.
	3	0	39	50	30,42		NW	1	Cloudy
3	8	0	36	47	30,38		W	1	Thick and cloudy.
	3	0	40	52	30,40		S	1	Cloudy
4	8	0	39	47	30,38		E	1	Cloudy, and foggy.
	3	0	40	58	30,36		E	1	Cloudy.
5	8	0	40	48	30,10		SSE	1	Cloudy.
	3	0	40	58	29,37		S	1	Cloudy.
6	8	0	38	48	29,57		W	1	Fine.
	3	0	43	53	29,50		W	1	Fine.
7	8	0	36	49	29,48		W	1	Fine.
	3	0	44	53	29,56		W	1	Fine.
8	8	0	35	49	29,56		W	1	Fine.
	3	0	40	49	29,61		W	1	Fine.
9	8	0	38	46	29,71		S	1	Fine.
	3	0	44	51	29,57		S	1	Rain.
10	8	0	40	49	29,60		W	1	Cloudy.
	3	0	42	53	29,58		S	1	Cloudy.
11	8	0	40	50	29,29		W	1	Cloudy.
	3	0	40	50	29,34		W	1	Fine.
12	8	0	36	48	29,44		SW	1	Cloudy.
	3	0	41	51	29,07		SE	1	Rain.
13	8	0	38	54	29,03		W	1	Cloudy.
	3	0	41	53	29,22		W	1	Fine.
14	8	0	36	49	29,41		W	1	Fine.
	3	0	51	53	29,46		SW	1	Fine.
15	8	0	41	49	29,86		Var.	1,2	Cloudy, a violent gale of wind in the night.
	3	0	42	49	29,05		WNW	1	Fine.
16	8	0	34	46	29,41		W	1	Fine.
	3	0	40	49	29,49		W	1	Fine.

METEOROLOGICAL JOURNAL
for December, 1816.

1816	Time. H.	Time. M.	Therm. without. °	Therm. within. °	Barom. Inches.		Winds. Points.	Winds. Str.	Weather.
Dec. 17	8	0	41	48	29,38	Rain this Month 1,741 Inches.	S	1	Rain.
	3	0	48	53	29,30		W	1	Fair.
18	8	0	40	49	29,31		W	1	Cloudy.
	3	0	43	52	29,47		N	1	Cloudy.
19	8	0	35	48	30,12		N	1	Fair.
	3	0	37	52	30,12		E	1	Snow.
20	8	0	30	46	30,49		N	1	Fair.
	3	0	39	52	30,53		N	1	Fine.
21	8	0	30	45	30,40		N	1	Foggy.
	3	0	33	48	29,27		E	1	Fine.
22	8	0	25	42	30,09			1	Foggy.
	3	0	29	42	30,09		S	1	Foggy.
23	8	0	32	40	30,04		W	1	Cloudy.
	3	0	40	47	29,90		W	1	Rain.
24	8	0	46	46	29,70		W	1	Cloudy.
	3	0	48	49	29,62		W	1	Cloudy.
25	8	0	38	48	29,78		W	1	Cloudy.
	3	0	40	49	29,79		S	1	Fine.
26	8	0	48	48	29,43		S	1	Rain.
	3	0	44	53	29,35		S	1	Rain.
27	8	0	40	47	29,49		S	1	Fine.
	3	0	40	51	29,43		S	1	Fine.
28	8	0	36	47	29,83		W	1	Fine.
	3	0	44	50	29,69		S	1.2	Cloudy.
29	8	0	42	48	29,49		SW	1.2	Fine, a violent gale in the night.
	3	0	47	49	29,65		W	1	Fine.
30	8	0	39	47	29,95		E	1	Rain.
	3	0	38	49	29,84		E	1	Rain.
31	8	0	45	49	29,71		SW	1	Rain.
	3	0	48	51	29,70		S	1	Cloudy.

1816.	Thermometer without.			Thermometer within.			Barometer.*			Rain.†
	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	Greatest height.	Least height.	Mean height.	
	Deg.	Deg.	Deg.	Deg.	Deg.	Deg.	Inches.	Inches.	Inches.	Inches.
January	48	25	38,8	54	41	48,2	30,45	28,92	29,67	0,733
February	55	19	39,0	59	30	49,4	30,38	28,88	30,92	1,625
March	53	32	41,[illegible]	[illegible]	47	51,8	30,34	29,02	29,78	0,425
April	64	35	47,3	67	45	54,3	30,11	29,07	29,69	1,020
May	67	41	53,3	65	51	57,9	30,15	29,18	29,81	0,902
June	70	48	58,2	69	53	60,7	30,10	29,24	29,89	0,931
July	69	52	58,8	70	56	61,6	29,92	29,33	29,65	2,789
August	69	53	61,0	69	50	61,1	30,24	29,43	29,90	1,176
September	69	53	61,3	69	56	61,6	30,21	29,73	29,96	0,824
October	69	37	52,6	64	50	56,6	30,20	29,17	29,79	0,888
November	53	26	41,6	59	43	51,8	30,63	29,83	29,65	2,120
December	51	25	39,5	54	40	48,9	30,64	29,03	29,74	1,741
Whole year			49,4			55,3			29,87	15,174

* The quicksilver in the bason of the barometer, is 81 feet above the level of low water spring tides at Somerset-house.

† The Society's Rain Gage is 114 feet above the same level, and 75 feet 6 inches above the surrounding ground.

Mean variation of the magnetic needle, June 1816, 24° 17′ 54″ West.

PHILOSOPHICAL

TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXVII.

PART II.

LONDON,

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MDCCCXVII.

CONTENTS.

PHILOSOPHICAL

TRANSACTIONS.

XIII. *Description of a thermometrical barometer for measuring altitudes.* *By the Rev.* Francis John Hyde Wollaston, *B. D. F. R. S.*

Read March 6, 1817.

HAVING had my attention drawn, some years ago, when engaged in experimental Lectures at Cambridge, to the variations in the heat of boiling water as corresponding with the changes in atmospherical pressure and the height of the barometer, I constructed several very sensible thermometers, for the purpose of ascertaining these variations with minuteness, and have been led on by my observations into making an instrument, which I believe may be useful in measuring heights with greater accuracy and convenience than the common barometer. This is not proposed as a new idea, for I find that FAHRENHEIT has suggested it in his "Barometri novi descriptio," Phil. Trans. Vol. 33, p. 179, and also Mr. CAVALLO, Phil. Trans. Vol. 71, p. 524. But the instrument is carried farther than had been done by them.

At first my thermometers were made with different scales

from the uncertainty how far sensibility might be carried in them. In one instance my thread was so fine, and I had made so large a bulb, that every degree on Fahrenheit's scale was equal to 10 inches, and by connecting different threads to different bulbs, I have had them of all varieties from that length to half an inch.

The instrument, with which I have made the greatest number of observations, has a scale of 3,98 inches to every degree; the thread, which is 22 inches long, was proved before its attachment to the bulb, and being found not cylindrical, the proper allowance has been every where made for the variations in the different parts of it. The degree was ascertained by comparison with a good thermometer at low temperature before closing the tube. The degrees I divided into 100 parts on the scale, and by a Vernier into 1000. This has been compared with a common barometer, the height being always corrected for temperature according to General Roy's Table in Phil. Trans. Vol. 67, p. 687. With this its agreement has been very close, after I had detected by means of it and had corrected two inaccuracies in my barometer, which would otherwise have escaped me. One was in the total length from the basin to the scale, and was ascertained by a comparison made by means of a thermometer between my own barometer and two excellent mountain barometers, by Troughton and Cary. The other appeared by a want of agreement at low barometrical heights, when they agreed well above. For this I was at a loss to account, till I conjectured that it might arise from the greater quantity of mercury then expelled from the tube of the barometer and rising into the upper part of the basin, where the wooden box from

being thicker would occasion a greater variation. By altering the quantity of mercury in the basin I removed this cause of inaccuracy, and the instruments have agreed equally well in all parts between 30,68 on barometer and 28,28. The result of the comparison is, that a difference of 1° FAHRENHEIT is occasioned by 0,589 on the barometer corrected. 30,603 corrected barometer = 213,367 thermometer, and 28,191 barometer = 209,263 thermometer. There will be variations from this general result when the difference below the mean heights is considerable; but I did not attempt the observation of them, as my barometer is not provided with an adjustment of the mercury in the basin, and this thermometer is, I think, of too long a scale.

By these trials being satisfied of the capability of the instrument, I have endeavoured to render it as portable as possible, for the farther purpose of measuring difference of altitudes, of which I found it very sensible; and will describe what I may call, following FAHRENHEIT and CAVALLO, a thermometrical barometer, and may venture to recommend for use.

Pl. VII. Fig. 1. represents the thermometer. The bulb A, one inch diameter, is blown thick and strong on a tube of thick glass, the bore of which is not material, say $\frac{1}{40}$ inch. It is better to make the bulb on a separate thick tube, and to join the fine thread afterwards, than to attempt to blow it sufficiently large and strong through the fine thread itself. The thickness and strength of the bulb in every part is essential to its not yielding. Close above the bulb a swell B is made to contain, *as near as may be*, whatever mercury expands out of the bulb between the common temperature and that of boiling water. If this be too small, the mercury contracts into

the bulb, and may change its place by a shake; if too large, a part of the mercury remaining in B may be detached by a shake, and occasion great inconvenience. To prevent that possible detachment, the long shape given in the figure is preferable to a spherical swell. A workman accustomed to blowing thermometers, though he will at first make it too large, will soon hit the size. When the metal is hot in this part, a slight pressure of the tube endwise will occasion a little thickening of the glass externally about the part C, which is of use for fixing the thermometer in its mounting.

For the fine thread, the tube D is chosen by comparison with other thermometers, such that if a bulb were blown on it of 0,4 inch in diameter, its scale would be about four inches between freezing and blood heat; that is, 16° to an inch; when this tube is fitted to a bulb of an inch diameter, its degrees will therefore be about an inch each. The tube is five inches long. Before the joining is made at E, the bulb is filled: and the upper end of D being broken off *abruptly* is joined by its edges to a small piece of tube F of the same external diameter, but of an open bore, so as to make a sort of bulb at the top by the cap F; a blown bulb will not answer the purpose for which it is designed, of detaching a globule of mercury from the thread, and retaining it apart for future use. The joining at E must be neatly made without any crevices in which either mercury or air may lodge, and with as little swell in the thread as possible; for if there be any thing that can be called bulb in that part which is protected by the mounting from the action of the heat, the thermometer will be long in feeling its whole expansion.

Before F is sealed, the bulb and swell and F together con-

taining an excess of mercury, boil the thermometer in water, and if the instrument be wanted for the measurement of a height of 5000 feet, let the whole cool down to 200; if for 10,000 feet let it cool to 190, drawing the mercury into the thread: and at that point hastily tilt off the mercury remaining in F, which may then be sealed while the whole is kept hot. On boiling it up again, the excess of mercury which rises into F will be detached from the thread by a gentle tap or two on the side with the nail, and will remain in the cap F for use when required.

For mounting the thermometer, GH, Fig. 2. is a circular plate, one inch in diameter, with a hollow half cylinder K rivetted firmly through it, of sufficient size to admit the lower tube of the thermometer to be bound firmly to it, so that there be no shake, and no reliance for steadiness on the more tender part of the tube above. The hole L fits the tube pretty closely.

Fig. 3. is a second circular plate 1,5 in diameter with two screw collars 1,15 diameter of the same external thread, and a hole M in its centre for the tube of the thermometer to pass through it. The holes L and M being opened conically in opposite directions, allow a little fine tow to be wound round the tube, and when the two plates are fixed together by screws passing through them, they close the tow round the tube, both to steady it and to prevent the escape of steam.

Fig. 4, is the scale, 5 inches long, 0,9 broad; between the two standards N O a length of 4,15 inches is divided into 100 parts, and by the Vernier into 1000, giving 241 parts to the inch; this was accidental, being occasioned originally by the thread of my adjusting screw, which assisted me in making the divisions. The scale is fixed down to the upper plate,

Fig. 3, within the collar, by screws passing through the flanch P, at the back of the scale. It would be well, that a piece of thick leather, or soft wood, should be screwed between these two, if it can be done with sufficient firmness, for the purpose of preventing the scale getting inconveniently hot.'

The adjusting screw, which carries the Vernier, is raised by the standards above the scale, and is placed opposite the centres of the plates in fig. 2 and 3, by which means the milled head Q goes better into the case hereafter described. The tube of the thermometer, when passed through the central holes in the two plates, turns by its bend to the left hand, and up the side of the scale, being slightly fastened to it at the top only, with a small piece of cork under it, to keep it clear from the scale. The Vernier has fixed to it, by means of a screw head, two pieces of thick paper laid upon each other, the one black and the other white, half of the outer paper being cut away straight, makes a line between the black and white, better than any that can be drawn for adjustment to the top of the mercurial thread.

Were I wanting another of these instruments, and employing a workman to construct it for me, I would have the whole length of the adjusting rod square, with a small piece sliding by hand on it for the larger alterations, and carrying a short screw for the finer movements, or the whole scale and movements might, I think, conveniently be made with tubes in the manner of the mountain barometers, and the thermometer would not in that case require the bend, but would run up the centre.

To the Vernier Mr. CARY has attached for me, with a joint, a small lens of one inch focus, which assists in observing the

height of the mercurial thread, and by having no lateral motion, confines the view to the same direction and prevents parallax.

In boiling, the bulb should be exposed to steam only, as being steadier in its heat than the water. My boiler is a tin cylinder 5,5 deep, 1,2 diameter, with an external cylinder 1,4 diameter for preventing the transmission of heat, the bottom only is single. The interior cylinder has a brass collar soldered into it, having an internal screw which fits to either of the external screws on the plate Fig. 3.; so that what is boiler when fixed below plate 3, becomes a case to protect the scale, when screwed to the upper side of that plate. The top of the external cylinder being closed into the same brass collar, becomes slightly conical, and is soldered to it. An opening of 0.2 diameter is made through both cylinders, immediately under the collar, as a vent for the steam from within, but is prevented from communicating with the annular space between the vessels, lest inconvenience should arise from water accidentally getting between them.

Another tin cylinder, 1,2 in diameter, and 2,1 deep, with a similar screw collar at top, forms a case for protecting the bulb when screwed to the under side of plate 3, and is also a measure for the quantity of water put into the boiler, which should not touch the bulb; it is here 1,25 below it.

For the purpose of rendering every thing requisite for use portable also with the thermometer, I made a stand for it, which is convenient and will readily be described. Round the outside of the boiler, and just below the conical closing of it, is soldered a ring of brass wire ST. Fitting to the conical top of the boiler is made another short cone of thick tin, which

may be fastened down by screwing the thermometer into the boiler; or, which is better, by a separate collar U for the purpose, to screw into the boiler, having the same internal screw above to receive the thermometer. This conical cap has a wire soldered round it at VWX, and on this wire turn by eyes at their ends seven wires, nine inches long and of sufficient strength. They are placed at six equal distances round the cap, two of them being placed close together. These wires by bearing on the ring ST are spread outward, and being connected by gores of thin linen, sewed between them from top to bottom, are prevented from spreading beyond a certain point and form a very steady base for the support of the whole instrument, and at the same time a bell tent to protect the lamp and boiler from the wind. The two wire legs, which are placed together, are not connected otherwise than by a hook at bottom, and will allow the tent to be opened at that part for examining and adjusting the lamp, while the instrument stands firm on the remaining legs. The lamp (fig. 6.) is a cylinder 1,8 in diameter, and 0,9 deep, having a tube in the centre to carry a wick, and a cover with six holes round it of 0,2 diameter each, and an opening in the middle 0,8 diameter; a copper pipe, 0,85 in diameter, and 1,1 long, turns over the opening by means of a hinge, and forms a chimney to prevent smoke, on the principle of Argand's lamp. I burn oil with a quantity of tallow added, to make it congeal. The lamp has a rod of strong wire fixed to its circumference within, and sliding in a tube YZ on the outside of the boiler. Fig. 7. represents the instrument on its stand for use.

To pack the instrument for carriage, the thermometer is secured by being screwed into its upper and under caps, and

is then enveloped with the bulb downward in the folds of the tent inverted. The lamp is put first into the case, and the other parts being thrust down afterwards, are kept very steady by the linen of the tent. The whole goes into a tin cylindrical box, two inches diameter and ten deep, and weighs 1lb. $4\frac{1}{2}$oz.

The scale of an inch to a degree is chosen, because on trying various threads, I have found that when extremely fine, it is almost impossible to give such strength to the bulb as to force the column of mercury accurately to the same height on repetition of the boiling, by reason of the resistance from friction in the tube. With an inch scale, the variations of the barometrical thermometer will be to those of the common barometer as 5 to 3, and the sensibility in this instrument is so fully sufficient, that the difference of temperature arising from the height of a common table is immediately perceptible. If more were wanted, either the thread must be finer, which would endanger the precision; or the bulb larger, the objections to which are obvious. Adhesion to the glass and friction must always have some small effect: and for the same reason that a common barometer is shaken on observation, this instrument, when boiled, should be tapped gently two or three times on the side, to free the mercury; when that is done, whether on the rising of the column to its height, or the falling of it after a forced expansion, it will with this sized thread come to the same place precisely.

Upon trying this thermometer when mounted, I found that a variation of 0,589 barometer, which was before ascertained = 1 Fahrenheit, would be equal to 233 parts on my scale = 0,97 inch; and an inch on barometer would produce a

variation of 395 parts, or 1,643 inch on thermometrical barometer. My whole scale of 1000 parts would therefore equal 2,52 of barometer, and comprehend all changes from 28,1 to 30,6, if wanted to compare its variations with those of a common barometer. Having observed also with a former thermometer mounted on this scale, that 1° Fahrenheit or 0,589 barometer, was equal to an elevation of about 530 feet, I reckon that as on my thermometrical barometer 500 parts from the top would correspond with 29,3 barometer, I have at any height from 29,3 upwards, the other 500 parts or more to be applied to the measurement of altitudes, which allowing 233 parts to 530 feet, will comprehend every thing that can be wanted in this part of the kingdom. This being convenient for all my purposes, the thermometer was to be set to this point. I therefore first drew all the mercury from the cap F into the tube, and then expanding it carefully and gradually out, shook off the globules at top, till it should stand on boiling at such height according to the state of the barometer at the time, that by computation the top and bottom of the scale should correspond with 30,6 and 28,1 respectively. It is for the shaking off this globule that the fine tube must be broken abruptly at top and end flat; and there is no fear of the globule being drawn down again into the tube, unless the whole column is expanded to the top and connected with the globule in F.

Yet this instrument, though adjusted now to my own particular use by the quantity of mercury in the thread, is capable of measuring any greater altitude, even Mt. Blanc or Chimboraço, under any barometrical circumstances, and the change for that purpose is much easier effected than would

be imagined. At the lowest station (having previously drawn into the tube the whole globule in F by expanding the mercury and connecting the thread at top) boil the thermometer; put a wooden peg into the steam vent, and by that forced expansion shake off a few parts at the top of the scale, so that the boiling point may be there accurately taken. Ascend till on boiling again the point is near the bottom of the scale, which will be with this particular instrument about 2200 feet. After an observation at this second station, adapt the thermometrical barometer to measuring in like manner another ascent to a third station, by drawing the globule in again, and expanding out the excess as before, so that the mercury may stand again at the top of the scale, and its point may be noted. Hence proceed to a fourth station, and so in like manner to others, getting the difference between the several stations, and consequently the aggregate height. The only correction requisite is for the specific gravity of air at different temperatures given by General Roy, Phil. Trans. 67, 770, for this a small thermometer is wanted, and will find room in the case among the folds of the linen tent.

The experiments, which I have been able to make on altitudes, have been few, and they were made with a different instrument, which was unluckily broken. It had more sensibility than that which I have described, 1° Fahrenheit being equal to 552 parts on my scale, or 2,3 inches. By a few observations, such as the height of my house afforded, I reckoned 552 parts equal to 530 feet in altitude. With that instrument, boiled on the counter of a bookseller's shop in Paternoster-row, which I estimated between four and five feet

above the foot pavement on the north side of St. Paul's church-yard, and boiled again in the Gilt Gallery, there was a difference of 254 parts, barometer being 29,92, thermometer 77. 552 parts : 530 feet :: 254 parts : 243,87 feet, to which add the correction by ROY's table $\frac{118}{1000}$ of the height $= 28.77$, and the corrected height is 272.64. General ROY makes the gallery above the north pavement to be 281 feet, which allowing 4 or 5 feet for the difference of our lower stations, would give 266 or 267 feet for my observed height, differing only by about 4 feet. If I take my proportion from ROY's statement that 1° Fahrenheit = 535 feet, the result will be still nearer. 552 : 535 :: 254 : 246,1, add correction $\frac{118}{1000} =$ 29, the corrected altitude will be 275,1 feet, differing less than 2 feet.

One other observation I made with the same thermometer on a height ascertained also by General ROY. My thermometer boiled at the ferry-house, opposite Woolwich arsenal, stood at 869, and in the Prospect-room at the Inn on Shooter's hill, it showed 432, a difference of 437 parts, barometer 29,94, thermometer 58.

$$\left.\begin{array}{r} 552 : 530 :: 432 : 419{,}6 \\ \text{Add correction } \frac{67{,}5}{1000} \quad = \quad 28{,}3 \end{array}\right\} \text{height corrected} = 447{,}9.$$

By ROY, the height from the Gun wharf at Woolwich to the upper story at Shooter's hill is 444 feet.

If this instrument should be thought deserving of being brought into use for scientific purposes, the sensibility and length of the thermometer would be selected by each person according to the particular object in view. With a sensibility equal to that of a common barometer, 1° of Fahrenheit

being equal to 0,589 inch, whether a scale of the common or thermometrical barometer, an equal precision may, I think, be attained; and with a power considerably increased, the instrument will always be much more portable than a common barometer, although the scale should be extended for taking the greatest known heights by a single pair of observations.

F. J. H. WOLLASTON.

Southweald, March 1, 1817.

P. S. Since the above account was written, two heights have been measured with the instrument there described, which agree with the measurements made by General Roy. March 21, 1817, the thermometrical barometer boiled in a house on the edge of the wharf at Scotland-yard, and on a level with the wharf, stood at 780, thermometer 41, barometer 29,98. Boiled on the same morning in the Dining-room at the Spaniard at Hampstead, it stood at 599—thermometer 37—on the road to Hampstead, thermometer 39—mean thermometer 39. Difference of stations 181. 233:535::181:415. Correction to be added $\frac{7\times2,52}{1000}$ of height=725 feet. Corrected height=422,25 feet. Made by General Roy 422 feet.

April 3, 1817. The thermometrical barometer boiled in Mr. Dolland's back shop in St. Paul's church-yard, 4 feet above the north pavement, stood at 886, barometer 30,41, thermometer 57. Out-doors below, and on Stone Gallery, and on Gilt Gallery of St. Paul's, thermomer 60, therefore thermometer taken at 60. In the Gilt Gallery, 2 feet above the floor, the

thermometrical barometer stood at 773,5. Difference = 112,4. 233 : 535 :: 112,4 : 258. Correction = $\frac{74.5}{1000}$ of height = 19,2 feet. Corrected height = 258 + 19,2 = 277,2 feet. General ROY's measurement, allowing for the difference of the stations, is 279 feet.

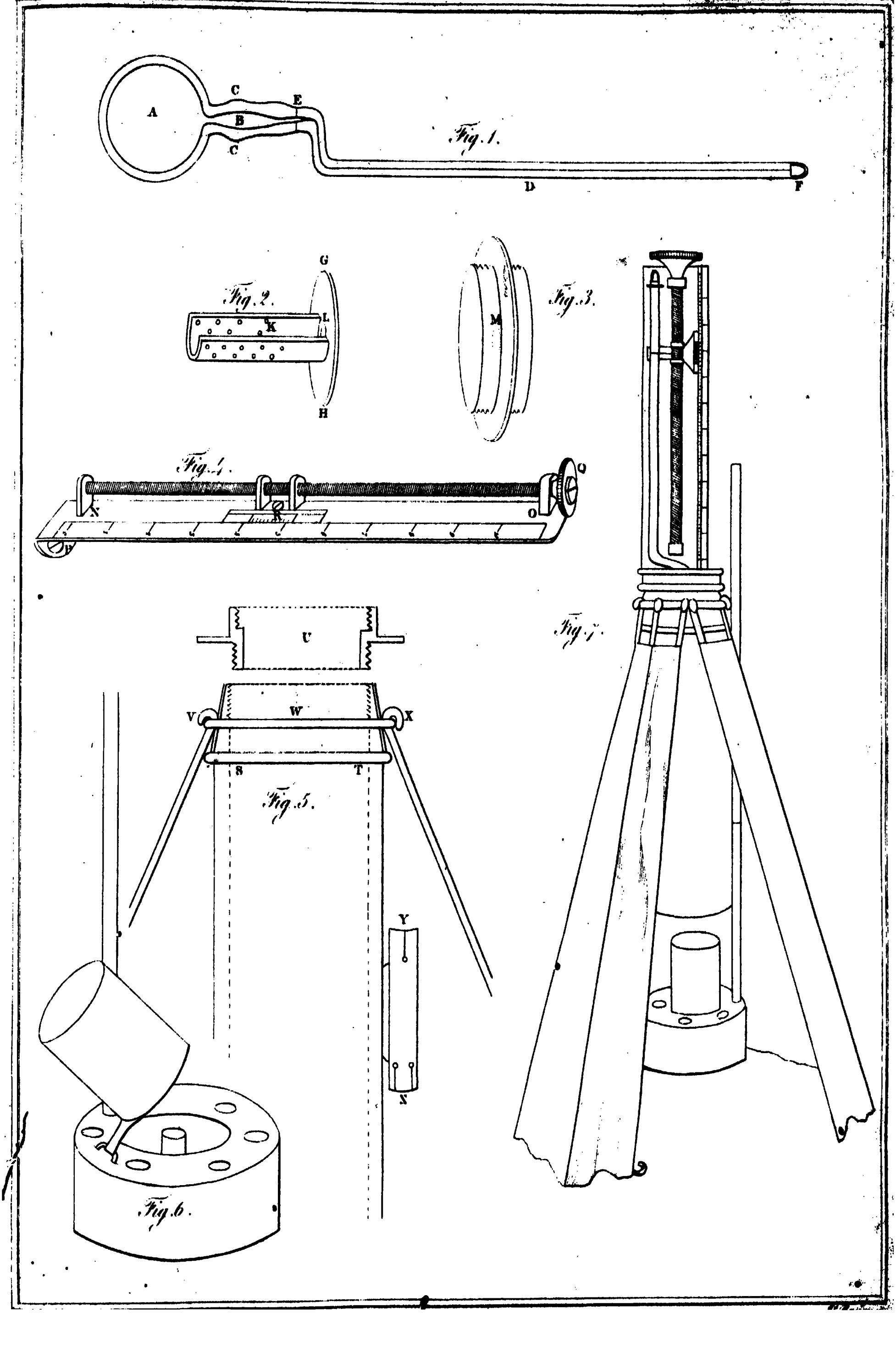
Fig. 1.
A
B
C
C
E
D
F
Fig. 2.
G
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H
Fig. 3.
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Fig. 4.
N
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T
Fig. 5.
Y
Z
Fig. 6.
Fig. 7.

XIV. *Observations on the Analogy which subsists between the Calculus of Functions and other branches of Analysis.* By Charles Babbage, *Esq. M. A. F. R. S.*

Read April 17, 1817.

It is my intention in the following Paper to offer to the Royal Society some remarks on the utility of analogical reasoning in mathematical subjects, and to illustrate them by some striking facts which have occurred to me, when comparing the calculus of functions with other modes of calculation with which mathematicians have been long acquainted. The employment of such an instrument may, perhaps, create surprise in those who have been accustomed to view this science as one which is founded on the most perfect demonstration, and it may be imagined that the vagueness and errors which *analogy*, when unskilfully employed, has sometimes introduced into other sciences, would be transferred to this.

It is, however, only as a guide to point out the road to discovery, that analogy should be used, and for this purpose it is admirably adapted.

It is usually more difficult to discover than to demonstrate any proposition; for the latter process we may have rules, but for the former we have none. The traces of those ideas which, in the mind of the discoverer of any new truth, connect the unknown with the known, are so faint, and his attention is so much more intensely directed to the object, than to

the means by which he attains it, that it not unfrequently happens, that while we admire the happiness of a discovery, we are totally at a loss to conceive the steps by which its author ascended to it.

From these considerations, I think it will appear, that any successful attempt to embody into language those fleeting laws by which the genius of the inventor is insensibly guided in the exercise of the most splendid privilege of intellect, would contribute more to the future progress of mathematical science than any thing which has hitherto been accomplished. Amidst the total absence of all attempts of this kind, the following illustrations of one of the most obvious assistants of the inventive faculty, will not, I hope, be considered useless, even though it should have no other effect than that of directing the attention of those who are engaged in mathematical enquiries, to this most interesting and important subject.

At our first entrance into algebra, one of the most remarkable circumstances which presents itself is, that some fractions which in certain cases apparently vanish, have in fact a finite value. Such is the fraction $\frac{a^x-b^x}{x}$ which when $x=o$ becomes $\frac{1-1}{o}=\frac{o}{o}$, and yet its real value is well known to be log. $\frac{a}{b}$.

Here then by assigning a certain value to a variable quantity which is capable of all degrees of *magnitude*, the expression apparently becomes illusory: let us now examine a parallel case in the Calculus of Functions. Take the expression $\frac{\psi x-\psi\frac{1}{x}}{\psi x}$, and let us suppose ψ to be an arbitrary charac-

teristic capable of assuming all varieties of *form*; then amongst these varieties we may have $\psi x = o$, and the expression becomes $\frac{o-o}{o}$.

In order to ascertain its real meaning, let us suppose

$$\psi x = v\varphi x + v^2\varphi_1 x + \&c.$$

which if we suppose $v = o$ gives $\psi x = o$

then $$\psi\frac{1}{x} = v\varphi\frac{1}{x} + v^2\varphi_1\frac{1}{x} + \&c.$$

and the expression becomes

$$\frac{\psi x - \psi\frac{1}{x}}{\psi x} = \frac{\left(\varphi x - \varphi\frac{1}{x}\right)v + \left(\varphi_1 x - \varphi_1\frac{1}{x}\right)v^2 + \&c.}{v\varphi x + v^2\varphi_1 x + \&c.} = \frac{\left(\varphi x - \varphi\frac{1}{x}\right) + \left(\varphi_1 x - \varphi_1\frac{1}{x}\right)v + \&c.}{\varphi x + v\varphi_1 x + \&c.}$$

which becomes when $v = o$ or $\psi x = o$

$$\frac{\psi x - \psi\frac{1}{x}}{\psi x} = \frac{\varphi x - \varphi\frac{1}{x}}{\varphi x}$$

where φx is quite arbitrary.

If we suppose ψx to become a symmetrical function of x and $\frac{1}{x}$, we have $\psi x = \psi\frac{1}{x}$ and our expression becomes $\frac{o}{\psi x}$ which actually does vanish in all cases except at the same time $\psi x = o$.

As a second example take the expression $\frac{f_1 x - fx \,.\, f_1\alpha x}{1 - fx \,.\, f\alpha x}$ which becomes $\frac{o}{o}$ if the two following equations hold true $fx \,.\, f\alpha x = 1$ and $f_1 x - fx \,.\, f_1\alpha x = o$, α being any given function, such that $\alpha^2 x = x$. In order to ascertain its real value in that case, let us suppose

$$f_1 x \text{ becomes } f_1 x + v\varphi x$$

and $$fx \text{ becomes } fx + v\varphi x$$

then $$f_1\alpha x \text{ will be } f_1\alpha x + v\phi\alpha x$$

and $$f\alpha x \text{ will be } f\alpha x + v\phi\alpha x$$

and these being substituted we have

$$\frac{(f_1 x - fx \,.\, f_1\alpha x) - (\phi_1 x - fx \,.\, \phi_1\alpha x - f_1\alpha x \,.\, \phi x)v - \phi x \,.\, \phi_1\alpha x \,.\, v^2}{(1 - fx \,.\, f\alpha x) - (fx \,.\, \phi\alpha x + f\alpha x \,.\, \phi x)v - \phi x \,.\, \phi\alpha x \,.\, v^2}$$

now in consequence of the two equations given above, the first term in both numerator and denominator vanishes, and dividing the remainder by v and then making $v = o$, we have for the value of the expression when $f_1 x - fx \,.\, f_1\alpha x = o$ and also $fx \,.\, f\alpha x = 1$

$$\frac{f_1 x - fx \,.\, f_1\alpha x}{1 - fx \,.\, f\alpha x} = \frac{\phi_1 x - fx \,.\, \phi_1\alpha x - f_1\alpha x \,.\, \phi x}{fx \,.\, \phi\alpha x + f\alpha x \,.\, \phi x}$$

where ϕ and ϕ_1 are arbitrary.

If we take the expression

$$\frac{f_1 x - fx \,.\, f_1\alpha x + fx \,.\, f\alpha x \,.\, f_1\alpha^2 x}{1 + fx \,.\, f\alpha x \,.\, f\alpha^2 x}$$

and if fx and $f_1 x$ are so assumed that the numerator and denominator both vanish, by a similar mode of treatment to that already pointed out, we shall find its real value to be

$$\frac{\phi_1 x - fx \,.\, \phi_1\alpha x - f_1\alpha x \,.\, \phi x - fx \,.\, f_1\alpha x \,.\, \phi\alpha x - f\alpha x \,.\, f_1\alpha x \,.\, \phi x - fx \,.\, f\alpha x \,.\, \phi_1\alpha^2 x}{f\alpha x \,.\, f\alpha^2 x \,.\, \phi x + fx \,.\, f\alpha^2 x \,.\, \phi\alpha x + fx \,.\, f\alpha x \,.\, f\alpha^2 x}$$

And similarly if we supposed both numerator and denominator of the fraction

$$\frac{f_1 x - fx \,.\, f_1\alpha x + \&c. \pm fx \,.\, f\alpha x \ldots f\alpha^{n-2}x \,.\, f_1\alpha^{n-1}x}{1 \pm fx \,.\, f\alpha x \ldots f\alpha^{n-1}x}$$

to vanish, it would yet retain a value containing arbitrary functions.

It will be needless to multiply examples, as the mode of

treating them is sufficiently obvious from those already given. It appears then, that as in common algebra an expression may become illusory from the *variable quantity* assuming a particular *value;* so in the doctrine of functions an expression may become likewise illusory by the *variable function* assuming a particular *form;* in the one case the real value may be a *constant quantity,* in the other it may be an *arbitrary function:* nor ought this circumstance of the appearance of an arbitrary function to surprise us, when we consider that (as for instance in the second example) it is not one form only of the function fx which will satisfy the equation $fx.f\alpha x = 1$, but any of the infinite variety of forms comprehended in the expression $fx = \left\{ \chi\,(\overline{x}, \overline{\alpha x}) \right\}^{x - \alpha x}$ and similarly for the values of $\underset{1}{f}x$. The circumstance of this species of vanishing fractions having an arbitrary function contained in their true value, is of considerable importance in the calculus of functions, as I shall now shortly endeavour to prove.

The Royal Society did me the honour to insert, in the last volume of their Transactions, a paper of mine, in which I gave a new method of solving all functional equations of the first order, and of a certain class by means of elimination, and I there stated that all solutions so obtained were only particular cases of the general solutions, and that they did not contain any arbitrary function.

Now it may be observed, that there are certain forms which may be assigned to the coefficients which render those solutions apparently infinite, yet that on farther consideration it appears, that the solution is in fact a vanishing fraction: in all such cases the process I have just pointed out will give the real solution which will contain an arbitrary function, so that it is in fact a general solution. Thus in the equation $\psi x + fx.$

$\psi\alpha x = f_1 x$ whose solution is $\psi x = \dfrac{f_1 x - fx \,.\, f_1\alpha x}{1 - fx \,.\, f\alpha x}$ if $fx \,.\, f\alpha x = 1$ it apparently becomes infinite, but by subtracting the equation $fx \,.\, \psi\alpha x + fx \,.\, f\alpha x \,.\, \psi\alpha^2 x = fx \,.\, f_1\alpha x$ (which is deduced from the former by putting αx for x and multiplying by fx) we have since $\alpha^2 x = x$ and also $fx \,.\, f\alpha x = 1$, $o = f_1 x - fx \,.\, f_1\alpha x$, and the solution becomes a vanishing fraction whose value is $\psi x = \dfrac{\phi_1 x - fx \,.\, \phi_1\alpha x - f_1\alpha x \,.\, \phi x}{fx \,.\, \phi\alpha x + f\alpha x \,.\, \phi x}$ let $\alpha x = -x$ and $fx = f_1 x = 1$ then the general solution of the equation $\psi x + \psi(-x) = 1$ is $\psi x = \dfrac{\phi_1 x - \phi_1(-x) + \phi x}{\phi(-x) + \phi x}$.

In a similar manner the solutions of the equations $\psi x - \psi(-x) = x$, and $\psi x = fx \,.\, \psi\alpha x$ if $fx \,.\, f\alpha x = 1$ when $\alpha^2 x = x$ are

$$\psi x = \frac{x\phi x - \phi_1 x + \phi_1(-x)}{\phi x + \phi(-x)} \text{ and } \psi x = \frac{\phi_1 x + fx \,.\, \phi_1\alpha x}{fx \,.\, \phi\alpha x + f\alpha x \,.\, \phi x}$$

Let $\psi x + \psi\alpha x = f_1 x$ if $\alpha^2 x = x$ this is only possible when $f_1 x = f_2(\overline{x}, \overline{\alpha x})$ then

$$\psi x = \frac{\phi x \,.\, f_2(\overline{x}, \overline{\alpha x})}{\phi x + \phi\alpha x}$$

Let $\psi x - \psi\alpha x = f_1 x$ and $\alpha^2 x = x$ this is only possible when $f_1 x = (x - \alpha x) \times f_2(\overline{x}, \overline{\alpha x})$, then

$$\psi x = \frac{(x - \alpha x)\, f_2(\overline{x}, \overline{\alpha x}) \,.\, \phi_1 x + \phi x + \phi_1\alpha x}{\phi x + \phi\alpha x}$$

The above are sufficient as examples, but the same reasoning has led me to the following curious proposition. *Whenever the method of elimination apparently fails, the real value of the vanishing fraction will give the general solution of the equation.* This principle puts us in possession of the general solutions of several classes of equations, for besides the cases

in which the solutions vanish from some particular values of the coefficients, *all equations which are homogeneous relative to the different forms of the unknown function* are comprehended in it, as are also *all equations which are symmetrical relative to the same quantities.*

There exists another class of equations nearly allied to those which are symmetrical relative to the different forms of the unknown functions, whose solution I shall now point out, chiefly with the view of giving another example of a method of reasoning which may frequently be employed with advantage in these inquiries, and also for the purpose of mentioning a remark respecting the elimination of variables in a certain class of algebraic equations which I do not recollect to have seen noticed. The class of functional equations to which I allude, are contained in the expression

$$F\left\{\psi x, \psi \alpha x, \ldots \psi \alpha^{n-1} x, \overline{x}, \overline{\alpha x}, \ldots \overline{\alpha^{n-1} x}\right\} = o$$

which for the sake of convenience may be written thus,

$$F_{1}\left\{\psi x, \psi \alpha x, \ldots \psi \alpha^{n-1} x, f x, f_{1} x, f_{2} x, \&c.\right\} = o$$

where $\alpha^n x = x$ and $f x, f_1 x, f_2 x$, &c. are any symmetrical functions of $x, \alpha x, \ldots \alpha^{n-1} x$.

In this equation none of the known functions $f x, f_1 x$, &c. are changed by the substitution of $\alpha x, \alpha^2 x, \ldots \alpha^{n-1} x$ for x; and since the form of the unknown function depends on that of those which are known, it follows that the form of the unknown function will not be changed by the substitution of $\alpha x, \alpha^2 x, \ldots \alpha^{n-1} x$ for x, or in other words we may suppose $\psi x = \psi \alpha x =$ &c. $= \psi \alpha^{n-1} x$, and consequently ψx will be determined by the equation

$$F_{1}\left\{\psi x, \psi x, \ldots \psi x, f x, f_{1} x, f_{2} x, \&c.\right\} = o$$

or from $\quad F\{\psi x, \psi x, .. \psi x, \overline{x}, \overline{\alpha x}, \overline{\alpha^2 x}, .. \overline{\alpha^{n-1}x}\} = o$

If there should exist any doubt respecting the accuracy of this reasoning, it may be confirmed by arriving at the same conclusion in rather a different manner. If in the given equation we substitute successively $\alpha x, \alpha^2 x, .. \alpha^{n-1}x$ for x, we shall have the following equations.

$$F\{\psi x, \quad \psi\alpha x, .. \psi\alpha^{n-1}x, \overline{x}, \overline{\alpha x}, ... \overline{\alpha^{n-1}x}\} = o$$

$$F\{\psi\alpha x, \quad \psi\alpha^2 x, ... \psi x, \quad \overline{x}, \overline{\alpha x}, .. \overline{\alpha^{n-1}x}\} = o$$

&c. &c.

$$F\{\psi\alpha^{n-1}x, \psi x, .. \psi\alpha^{n-2}x, \overline{x}, \overline{\alpha x}, .. \overline{\alpha^{n-1}x}\} = o$$

To eliminate $\psi\alpha x$, $\psi\alpha^2 x$ &c. from these n equations would in most cases be excessively troublesome. It may however be observed, that $\psi\alpha x$ occurs in the second exactly in the same manner that ψx occurs in the first; also $\psi\alpha^2 x$ is contained in the third in the same manner as ψx is contained in the first, and similarly with the rest. From this it follows, that though no one of the equations is symmetrical relative to $\psi x, \psi\alpha x ... \psi\alpha^{n-1}x$, yet when all the equations are considered together, they are symmetrical relative to $\psi x, \psi\alpha x, ... \psi\alpha^{n-1}x$; from this it follows that whether we find by elimination $\psi x, \psi\alpha x, ..$ or $\psi\alpha^{n-1}x$ the result will be the same, therefore $\psi x = \psi\alpha x =$ &c. $= \psi\alpha^{n-1}x$, and we may determine ψx from the equation

$$F\{\psi x, \psi x, ... \psi x, \overline{x}, \overline{\alpha x}, ... \overline{\alpha^{n-1}x}\} = o \qquad (a)$$

It does not follow from hence, that this equation contains all the values of ψx: on the contrary, if the elimination between the n equations above written were actually performed, it would be found that the equation (a) would enter into the final result as one of its factors.

If we apply this reasoning to algebraic equations containing several variables, as for instance to the two equations

$$F(x, y, a, b) = o, \quad F(y, x, a, b) = o$$

we shall find that one set of the value of x is always contained in the equation $F(x, x, a, b) = o$.

As an example, let us take the two equations

$$y^2 + x = a \text{ and } x^2 + y = a$$

one set of the values of x are contained in the equation

$$x^2 + x - a = o$$

and this enters as a factor in the result of elimination, which gives

$$x^4 - 2ax^2 + x + a^2 - a = (x^2 + x - a)\,(x^2 - x - \overline{a - 1}) = o$$

Another curious analogy exists between the calculus of functions and common algebra in the similarity of the relations of the roots of unity to the solutions of the functional equation $\psi^n x = x$.

In the equation $r^n = 1$ it is known that if r be one of the roots then any power of r will also be a root, and if n is a prime number and r any root except unity, then $r, r^2, r^3, \ldots r^{n-1}$ will be all the different roots. Similarly in the functional equation $\psi^n x = x$ if αx be one form which satisfies it, $\alpha^m x$ will also fulfill it, and if n is a prime number then $\alpha x, \alpha^2 x, \ldots \alpha^{n-1}$ will all be different forms which satisfy the equation. This may be readily shown as follows, if αx is a solution, then

$$\alpha\alpha \ldots (n)\, x = \alpha^n x = x$$

Suppose $m = 2$ then

$$\alpha^2 \alpha^2 \ldots (n)\, x = \alpha^{2n} x = \alpha^n (\alpha^n x) = \alpha^n x = x$$

consequently $\alpha^2 x$ is also a solution of $\psi^n x = x$

again $\alpha^m \alpha^m \ldots (n) x = \alpha^{n \times m} = \alpha^n \alpha^n \ldots (m) x =$
$= \alpha^n \alpha^n \ldots (m-1) x = \&c. = \alpha^n \alpha^n x = \alpha^n x = x$
consequently whenever m is a whole number $\alpha^m x$ is a solution of $\psi^n x = x$.

If we take the particular case of $n = 3$ we have $\psi^3 x = x$ and $\alpha x = \frac{1}{1-x}$ is one of its solutions, therefore other solutions will be $\alpha x = \frac{1}{1-x}$, $\alpha^2 x = \frac{x-1}{x}$, $\alpha^3 x = x$.

These expressions when generalised by the introduction of an arbitrary function, do not give solutions which are irreducible to each other, nor do they even then in my opinion contain all possible solutions; by introducing an arbitrary function they become

$$\overset{-1}{\varphi}\left(\frac{1}{1-\varphi x}\right) \text{ and } \overset{-1}{\varphi}\left(\frac{\varphi x - 1}{\varphi x}\right)$$

the latter of which gives $\frac{1}{1-x}$ if we make $\varphi x = \frac{1}{x}$.

If we consider the equation $\psi^6 x = x$ one of its values is $\alpha x = \frac{3x-3}{x}$, this gives for the others.

$$\alpha x = \frac{3x-3}{x}, \ \alpha^2 x = \frac{2x-3}{x-1}, \ \alpha^3 x = \frac{3x-6}{2x-3}$$
$$\alpha^4 x = \frac{x-3}{x-2}, \ \alpha^5 x = \frac{-3}{x-3}, \ \alpha^6 x = x$$

All these forms will satisfy the equation $\psi^6 x = x$ and they may all be generalised by the introduction of an arbitrary function similar to that employed for the equation $\psi^3 x = x$, but I do not suppose these solutions when thus generalised would contain all possible ones.

Perhaps the following observations may throw some light on the generality of the solutions of such equations as those we are now considering.

In the first place, it is obvious that every solution of $\psi^3 x = x$

will also be a solution of $\psi^6 x = x$, such likewise will be all the solutions of $\psi^2 x = x$. The complete solution of $\psi^6 x = x$ should therefore contain all forms of x which satisfy the equations $\psi^3 x = x$ and $\psi^2 x = x$. Again, if a function as αx satisfies the equation $\psi^3 x = -x$, it will also satisfy $\psi^6 x = x$, for since $\alpha^3 x = -x$ by putting $\alpha^3 x$ for x it becomes $\alpha^3 \alpha^3 x = -\alpha^3 x = -(-x) = x$, so also any function which satisfies the equation $\psi^3 x = \frac{a}{x}$ will fulfil the equation $\psi^6 x = x$ which may be proved in the same manner. I shall however take the more general case, and show that if any function satisfies the equation $\psi^3 x = \beta x$ where β is any function satisfying the equation $\psi^2 x = x$, it will also satisfy the equation $\psi^6 x = x$, for since $\alpha^3 x = \beta x$, putting $\alpha^3 x$ for x, we have $\alpha^3 \alpha^3 x = \alpha^6 x = \beta \alpha^3 x = \beta\beta x = \beta^2 x = x$. In the same way it might be demonstrated, that the complete solution of $\psi^{abc, \&c.}(x) = x$ must contain all functions which can satisfy any of the following conditions

$$\begin{array}{ccc} \psi^a x = \alpha x & \psi^{ab} x = \alpha_1 x & \&c. \\ \psi^b x = \beta x & \psi^{bc} x = \beta_1 x & \&c. \\ \&c. & \&c. & \end{array}$$

where α is any	function satisfying	the equation	$\psi^{bc\ \&c.}(x) = x$
β	ditto	ditto	$\psi^{ac\ \&c.}(x) = x$
&c.	&c.		
α_1	ditto	ditto	$\psi^{cd\ \&c.}(x) = x$
β_1	ditto	ditto	$\psi^{ad\ \&c.}(x) = x$
&c.	&c.		&c.

The comparison of the integral calculus with that of functions will supply us with several very marked analogies, some of which promise when farther pursued to be of essen-

tial service in the improvement of this latter branch of an a lytical science. One of the first which presents itself is the method of solving the differential equation

$$o = ydx^n + \underset{x}{A}dx^{n-1}dy + \underset{x}{B}dx^{n-2}d^2y + \&c. + \underset{x}{N}d^ny + Xdx^n$$

compared with that of solving the functional equation

$$o = \psi x + \underset{x}{A}\psi\alpha x + \underset{x}{B}\psi\alpha^2 x + \&c. + \underset{x}{N}\psi\alpha^{n-1}x + X$$

Euler and D'Alembert succeeded in integrating the differential equation when all the coefficients except X are constant quantities, and Lagrange, in the Memoirs of the Academy of Berlin, 1772, explained a method of treating the same when they are variable; both the processes alluded to depend in the first instance on reducing the equation to the solution of the same equation, wanting the last term; and this is effected by means of a particular integral of the given equation.

The solution of the functional equation is precisely similar, it is first reduced to the solution of

$$o = \psi x + \underset{x}{A}\psi\alpha x + \underset{x}{B}\psi\alpha^2 x + \&c. + \underset{x}{N}\psi\alpha^{n-1}x$$

and this is effected by knowing a particular function which satisfies the original equation: this process I have already given in a former paper. The resemblance between the solutions of the two equations continues also in this respect, if fx is a particular case of the given equation, and Kx, $K_1 x$, $K_2 x$, &c. are particular cases of the same equation without its last term, then

$$\psi x = fx + Kx \cdot \chi(\overline{x}, \overline{\alpha x}, \ldots \overline{\alpha^{n-1}x}) + K_1 x \cdot \chi_1(\overline{x}, \overline{\alpha x}, \ldots \overline{\alpha^{n-1}x}) + \&c.$$

is a general solution of the equation.

It may be observed that the functional equation under consideration comprehends a large class of others which may easily be reduced to it, such as

$$F\psi x + \underset{x}{A}\, F\psi\alpha x + \underset{x}{B}\, F\psi\alpha^2 x + \&c. + \underset{x}{N}\, F\psi\alpha^{n-1}x + X = 0$$

or more generally

$$F(x, \psi x) + \underset{x}{A}\, F(\alpha x, \psi\alpha x) + \underset{x}{B}\, F(\alpha^2 x, \psi\alpha^2 x) + \&c.$$
$$+ \underset{x}{N} F(\alpha^{n-1}x, \psi\alpha^{n-1}x) + X = 0$$

both of which may be reduced to the same equations wanting the last term, these transformations apply with advantage to a great variety of other equations: the solution of the latter of these equations may be reduced to that of

$$\underset{1}{\psi} x + \underset{x}{A}\underset{1}{\psi}\alpha x + \&c. + \underset{x}{N}\underset{1}{\psi}\alpha^{n-1}x = 0$$

and if we find $\underset{1}{\psi} x = Kx$ we shall have

$$\psi x = F^{1,-1}(x, Kx)$$

for the solution of the given equation.

As an instance of the utility of the former equation we may take $$\left\{\psi(x)\right\}^2 + \left\{\psi\left(\frac{\pi}{2} - x\right)\right\}^2 = 1$$

whose solution will be found by that method to be

$$\psi x = \sqrt{\frac{1}{2} + \left(2x - \frac{\pi}{2}\right)\chi\left(x, \frac{\pi}{2} - x\right)}$$

the same equation or the more general one

$$\left\{\psi x\right\}^n + \left\{\psi\alpha x\right\}^n = 1$$

where $\alpha^2 x = x$ may be solved by another process which requires the aid of vanishing fractions, and we shall have

$$\psi x = \left\{\frac{\varphi x - \varphi\alpha x + \underset{1}{\varphi} x}{\underset{1}{\varphi} x + \underset{1}{\varphi}\alpha x}\right\}^{\frac{1}{n}}$$

The analogy between the various orders of functions which contain many variables, and that branch of the integral calculus which relates to *partial differentials*, is too apparent to require illustration. I shall therefore proceed to show that a functional equation admits of three species of solutions. 1st. *The complete solution, this contains as many arbitrary functions as the nature of the given equation will admit.* 2d. *The particular case, this contains all solutions which are less general than the complete solution, and which are only particular cases of it.* If they contain arbitrary functions, I have called them, for the sake of convenience, *general solutions.* 3d. *The particular solution, this is a solution which satisfies the equation, and may or may not contain arbitrary functions; its peculiar property is, that it is found from one part only of the equation and independent of the rest;* thus if we have the equation

$$\left\{\psi^{1,2}(x,y)-y\right\} . F\left\{x,y,\psi(x,y),\psi^{2,1}(x,y) \text{ \&c.}\right\}$$
$$=\left\{\psi^{\overline{2,2}}(x,y,)-\overset{-1}{\varphi}(o)\right\} . \underset{2}{F}\left\{x,y,\psi(x,y), \text{\&c.})\right\}$$

$\psi(x,y)=\overset{-1}{\varphi}(\varphi x-\varphi y)$ is a particular solution, for it satisfies the equation by making $\psi^{2,1}(x,y)-y=o$ and $\psi^{\overline{2,2}}(x,y)-\overset{-1}{\varphi}(o)=o$, and is totally independent of the rest of the equation contained in the given function F and $\underset{2}{F}$, provided only that it does not make either of them infinite, whether it is contained in the complete solution, I have not yet ascertained; but I am rather of opinion, that it will be found not to be included in it. In both parts of my Essay towards the Calculus of Functions, I have used the expression, particular solution, instead of particular case, this arose from not having taken a sufficiently extensive view of the calculus; it would

perhaps be desirable to confine the meaning of *particular solution* to the definition which has just been given. It is needless to add that the different species of solutions just enumerated, bear a strong resemblance to those of differential equations.

Amongst differential equations containing more than two independent variables, a large proportion do not admit of any integrals, these can only be integrated by assigning some relation between the variables, an analogous case occurs with respect to functional equations, a large number of those which contain two or more variables admit of no solution, unless we assign some relation between the variable quantities; examples of such equations may be found in the second part of the Essay towards the Calculus of Functions. And here perhaps it may not be misplaced to state a difficulty of a peculiar nature with respect to functional equations which are impossible; for the sake of perspicuity I shall consider a very simple case

$$\psi x = c\psi \frac{1}{x}$$

for x substitute $\frac{1}{x}$ $\qquad \psi \frac{1}{x} = c\psi x$

and by multiplication $\psi x \times \psi \frac{1}{x} = c^2 \psi \frac{1}{x} \times \psi x$ or $1 = c^2$ from which it follows that $c = \pm 1$ or in other words that the equation $\psi x = c\psi \frac{1}{x}$ is contradictory unless $c = \pm 1$. Now the functional equation $F\left\{x, \psi x, \psi \alpha x\right\} = 0$ has been reduced by LAPLACE by means of a very elegant artifice to an equation of finite differences, nor am I aware that this profound analyst has pointed out any restriction or any

impossible case: if we treat the equation $\psi x = c\psi x^n$ by his method, we shall find for its solution

$$\psi x = c^{\frac{-\log\log x}{\log n}}$$

and this solution satisfies the equation $\psi x = c\psi x^n$ independently of any particular value of n, and if we suppose $n = -1$

we have $$\psi x = c^{\frac{-\log^2 x}{\log n}}$$

for the solution of the equation $\psi x = c\psi \frac{1}{x}$ whatever may be the value of c, and we have before shown that it cannot have a solution unless $c = \pm 1$. The only explanation I am at present able to offer concerning this contradiction, is one which I hinted at on a former occasion, viz. that if we suppose ψ to represent any inverse operation which admits of several values, then if throughout the whole equation we always take the same root or the same individual value of ψ, it is impossible to satisfy the equation, but if we take one value of ψ in one part, and another of the values of ψ in other parts of the equation, it is possible to fulfil it by such means. This solution may perhaps appear unsatisfactory, it is however only proposed as one which deserves examination, and I shall be happy if its insufficiency shall induce any other person to explain more clearly a very difficult subject.

One of the most extensive methods of integrating differential equations, consists in multiplying by such a factor as will render the whole equation a complete differential, the determination of this factor is, however, generally a matter of at least equal difficulty with that of solving the original equation: analogy would lead us to suspect that some similar mode might be adopted for the solution of functional equa-

tions, varying of course in a certain degree from the difference of the objects to be obtained: the theory which I am now to explain will show that this suspicion is not without foundation, and will at the same time unfold one of the most beautiful parallels between the integral and functional calculus which I have yet observed. It has been already shown, that when an equation is symmetrical relative to the different forms of the unknown function, as

$$F\left\{x, \overline{\psi x}, \overline{\psi\alpha x}, \ldots \overline{\psi\alpha^{n-1}x}\right\} = 0$$

the method of solution by elimination apparently becomes illusory, but that the general solution of the equation may be deduced from the vanishing fraction to which this method then leads. If therefore we can make any functional equation symmetrical, relative to the different forms of the unknown functions, we can then obtain its general solution. Now this may be effected by multiplying the equation by some factor, and determining this factor, so that the result shall be symmetrical. The discovery of this factor (as in the case of a differential equation) requires the solution of a functional equation of several variables, but fortunately the class of equations to which they belong are not of very great difficulty.

To begin with a very simple case, let us consider the equation $\quad \psi x + fx \,.\, \psi\alpha x = \underset{1}{f}x$ where $\alpha^2 x = x$

multiplying by ϕx we have

$$\phi x \,.\, \psi x + \phi x \,.\, fx \,.\, \psi\alpha x = \phi x \,.\, \underset{1}{f}x$$

now the first side of this equation will be symmetrical relative to ψx and $\psi\alpha x$, if we make

$$\phi x = f\alpha x \,.\, \phi\alpha x$$

this equation can only be possible when $fx \,.\, f\alpha x = 1$ or when

$$fx = \left\{\chi(\bar{x}, \overline{\alpha x})\right\}^{x-\alpha x}$$

and supposing $fx \,.\, f\alpha x = 1$, we have $\phi x = \frac{1}{\sqrt{fx}}$ consequently the equation becomes

$$\frac{\psi x}{\sqrt{fx}} + \sqrt{fx} \,.\, \psi\alpha x = \frac{\psi x}{\sqrt{fx}} + \frac{\psi\alpha x}{\sqrt{f\alpha x}} = \frac{\underset{1}{f}x}{\sqrt{fx}}$$

and since the left side of this equation is symmetrical, the right side must also be so, consequently

$\frac{\underset{1}{f}x}{\sqrt{fx}} = \underset{2}{f}(\bar{x}, \overline{\alpha x})$ or the given equation is impossible, unless

$$\underset{1}{f}x = \sqrt{fx} \,.\, \underset{2}{f}(\bar{x}, \overline{\alpha x})$$

and the solution of the equation will be found by taking the value of a vanishing fraction to be

$$\psi x = \frac{\sqrt{f\alpha x} \,.\, \underset{2}{f}(\bar{x}, \overline{\alpha x}) \,.\, \phi x - \underset{1}{\phi}x + fx \,.\, \underset{1}{\phi}\alpha x}{fx \,.\, \phi\alpha x + f\alpha x \,.\, \phi x}$$

This method is applicable to all equations of the first degree, but I shall now point out a principle which extends to all equations of the first order, and which may probably be applied with some modification to those of higher orders.

Let us consider the equation

$$\mathrm{F}\left\{x, \alpha x, \psi x, \psi\alpha x\right\} = 0, \text{ where } \alpha^2 x = x$$

multiply this by $\Phi\left\{x, \alpha x, \psi x, \psi\alpha x\right\} = 0$

it becomes

$$\mathrm{F}\left\{x, \alpha x, \psi x, \psi\alpha x\right\} \times \Phi\left\{x, \alpha x, \psi x, \psi\alpha x\right\} = 0$$

in order that this equation may be symmetrical, we must have

$$\mathrm{F}\left\{x, \alpha x, \psi\alpha x\right\} \times \Phi\left\{x, \alpha x, \psi x, \psi\alpha x\right\}$$
$$= \mathrm{F}\left\{\alpha x, x, \psi\alpha x, \psi x\right\} \times \Phi\left\{\alpha x, x, \psi\alpha x, \psi x\right\}$$

from which the form of ϕ must be determined, one value is

$$\phi(x, \alpha x, \psi x, \psi\alpha x) = F\{\alpha x, x, \psi\alpha x, \psi x\}$$

hence the equation

$$F\{x, \alpha x, \psi x, \psi\alpha x\} \times F\{\alpha x, x, \psi\alpha x, \psi x\} = o$$

is symmetrical, and its general solution may therefore be found. It must however be observed, that all the solutions of this latter equation are not necessarily solutions of the former one, and it may be a matter of some difficulty to ascertain what solutions ought to be excluded. There is no part of this process which limits it to the particular case we have considered, and if the given equation were

$$F\{x, \psi x, \psi\alpha x, \ldots \psi\alpha^{n-1}x\} = o$$

we might take the equation

$$\phi\left\{\overline{F\{x, \psi x, \psi\alpha x, \ldots \psi\alpha^{n-1}x\}}, \overline{F\{\alpha x, \psi\alpha x, \ldots \psi x\}}, \ldots \right.$$
$$\left. \ldots\ldots\ldots\ldots \overline{F\{\alpha^{n-1}x, \psi\alpha^{n-1}x, \ldots \psi\alpha^{n-2}x\}}\right\} = o$$

which is symmetrical relative to ψx, $\psi\alpha x$, &c. $\psi\alpha^{n-1}x$.

I shall not at present enter into any farther details respecting this mode of solving functional equations, as it forms the subject of some investigations on which I am now engaged, and which are as yet incomplete.

In the preceding pages I have endeavoured to point out some of the more prominent points in which the calculus of functions resembles common algebra or the integral calculus: it ought however to be observed, that several of the methods which I have applied to the solution of functional equations, directly resulted from pursuing this analogy: for when I had ascertained the remarkable similitude which exists between

the method of functions and the integral calculus, I referred to a treatise on that subject with the express purpose of endeavouring to transfer the methods and artifices employed in the latter calculus, to the cultivation and improvement of the former.

CHARLES BABBAGE.

March 5, 1817.

XV. *Of the construction of Logarithmic Tables.* *By* Thomas Knight, *Esq.* *Communicated by* Taylor Combe, *Esq.* *Sec. R. S.*

Read February 27, 1817.

1. I HAVE endeavoured, in this short Paper, to give a *simple* and *connected* theory of the construction of logarithms, which I think has not hitherto been done.

PROP. I.

To find the Logarithm of $1 + x$.*

It is not difficult to see that we may assume

$L(1+x) = {}'Ax + {}''Ax^2 + {}'''Ax^3 + {}''''Ax^4 + \&c.$, whence

$L(1+y) = {}'Ay + {}''Ay^2 + {}'''Ay^3 + {}''''Ay^4 + \&c.$, and

$L\left\{(1+x)(1+y)\right\} = L(1+x+y+xy)$, or putting $1+x=\pi$,

$= L\left\{1+(x+\pi y)\right\} = {}'A(x+\pi y) + {}''A(x+\pi y)^2 + {}'''A(x+\pi y)^3 + \&c.$

If we substitute these three expansions in the equation

$$L(1+x) + L(1+y) = L\left\{(1+x)(1+y)\right\}$$

which expresses the nature of logarithms, and compare the coefficients of the first power of y, we find

$${}'A = {}'A\pi + 2\,{}''A\pi x + 3\,{}'''A\pi x^2 + 4\,{}''''A\pi x^3 + \&c.$$

or
$$\begin{array}{l} {}'A = {}'A + 2\,{}''A \\ \phantom{{}'A = {}'A} + {}'A \end{array}\Bigg| \, x + \begin{array}{l} 3\,{}'''A \\ + 2\,{}''A \end{array}\Bigg| \, x^2 + \begin{array}{l} 4\,{}''''A \\ + 3\,{}'''A \end{array}\Bigg| \, x^3 + \&c.$$

whence, by comparing the coefficients of the powers of x,

${}'A = {}'A$, $2\,{}''A + {}'A = 0$, $3\,{}'''A + 2\,{}''A = 0$, $4\,{}''''A + 3\,{}'''A = 0$, &c.; or

${}'A = {}'A$, ${}''A = -\frac{{}'A}{2}$, ${}'''A = -\frac{2\,{}''A}{3} = \frac{{}'A}{3}$, ${}''''A = -\frac{3A'''}{4} = -\frac{{}'A}{4}$, &c.

* I find that the method of expansion made use of in this Proposition had been previously employed by Mr. SPENCE.

and $$L(1+x, = 'A\left\{\frac{x}{1} - \frac{x^2}{2} + \frac{x^3}{3} - \frac{x^4}{4} + \right\}$$

As for 'A it may be evidently taken at pleasure; and innumerable systems of logarithms may be formed by assigning different values to it, for

$$'A\,L(1+x) + 'AL(1+y) = 'AL\{(1+x)(1+y)\}$$

which expresses that, if every logarithm in a system be multiplied by the same constant quantity, the products will still form a system of logarithms to the same numbers.

Cor. By an easy transformation of $L(1+x)$, we get for BRIGG's logarithms, M being the modulus,

$L.\frac{a}{b} = 2M\left\{\frac{a-b}{a+b} + \frac{1}{3}\left(\frac{a-b}{a+b}\right)^3 + \frac{1}{5}\left(\frac{a-b}{a+b}\right)^5 + \right\}$; and whenever the logarithm of a fraction is spoken of in the following proposition, it is supposed to be found by this series.

2. How are we to begin, in forming a table of logarithms?

DELAMBRE (Preface to BORDA, p. 75) says, that we should begin at 10,000; and the same writer (Mémoires de l'Institut, Tome cinq. p. 65), speaking of the great French Tables, says that the logarithms of primes under 10,000 were calculated directly by series, and those of numbers above 10,000 by six orders of differences.

Now it is not easy to see, why any of the logarithms in the lower half of the Table, except those of the numbers 2 and 3, should be computed directl since they may be got, each by a single subtraction, from those in the upper half. Suppose, for instance, there had been found directly the logarithms of numbers from 100,000 to 200,000; those of numbers down to 50,000 are found by merely subtracting the logarithm of 2, successively, from those of all the even numbers;

beginning at the top of the Table, with L. 1999998, L. 1999996, &c., and setting down the remainder for the logarithms of the successive numbers below 100,000, viz. L. 99999, L. 99998, &c.

When we have got down to 50,000, if we were to proceed in the same way, we should have to operate on the logarithms thus obtained, between 100,000 and 50,000: If, therefore, we fear any accumulation of errors, we may (because $3 \times 49999 = 149997$) subtract the logarithm of 3 from L. 149997, and from the logarithm of every third number going downward, and set the remainders down successively for the logarithms of numbers below 50,000. And thus we may proceed till we get somewhat below 34,000; then the logarithm of 4 will carry us down to 25,000; and the logarithm of 5 to 20,000, which completes the work, those below 20,000 having been already found.

In the great French Tables, however, it has been thought proper to calculate the logarithms of numbers under 10,000 with more decimal places than the rest. These must necessarily be found independently of the others; as they form in reality a separate Table.

In the next proposition, is contained a general method of finding converging series for the calculation of logarithms. The propositions which follow this are only corollaries from it, and give forms for interpolation; so that *every thing* relating to the construction of logarithms is effected by one simple and uniform process.

PROP. II.

3. *To express a number* (x) *by the product of a series of fractions converging continually towards unity.*

Let n, n', n'', &c. be numbers much less than x; in the equation $x = x$, change x, in the second member, into $x + n$, and multiply by such a factor as will restore the equality; there arises $x = (x + n) \times \frac{x}{x+n}$. If, in the second member of this equation, we change x into $x + n'$, in the *last factor* $\frac{x}{x+n}$, and multiply by such a fractional factor as shall again restore the equality, we have

$$x = (x + n) \times \frac{x+n'}{x+n+n'} \times \frac{x(x+n+n')}{(x+n)(x+n')}.$$

If here, in like manner, we change x into $x + n''$ in the last factor, and restore the equality as before, by annexing a new factor, then

$$x = (x + n) \times \frac{x+n'}{x+n+n'} \times \frac{(x+n'')(x+n+n'+n'')}{(x+n+n'')(x+n'+n'')} \times \frac{x(x+n+n')(x+n+n'')(x+n'+n'')}{(x+n)(x+n')(x+n'')(x+n+n'+n'')}$$

and the same process may be repeated as long as we think it necessary. Now it is plain that the last annexed factor, as we continue these operations, must always approach nearer to unity than that which was the last before; thus, n being very small compared with x, $\frac{x}{x+n}$ does not much differ from unity, and when $x + n'$ is put for x in this fraction (n' being also very small compared with x) its value will be nearly the same as before: of course the annexed factor $\frac{x(x+n+n')}{(x+n)(x+n')}$ will differ very little from unity: and it will differ from it

much less than the preceding factor $\frac{x}{x+n}$; for let $\frac{x}{x+n}=1-\mu$, $\frac{x+n'}{x+n+n'}=1-\mu'$, and μ and μ' being small fractions; the new factor $\frac{x(x+n+n')}{(x+n)(x+n')}=\frac{1-\mu}{1-\mu'}=1-(\mu-\mu')$ nearly: and consequently differs less from unity than the factor which was last before.

4. These equations, being put into logarithms, give a series of converging expressions for the logarithm of x. We have successively,

1st. $\mathrm{L}.x=\mathrm{L}(x+n)+\mathrm{L}\left(\frac{x}{x+n}\right)$

2d. $\mathrm{L}.x=\mathrm{L}(x+n)+\mathrm{L}(x+n')-\mathrm{L}(x+n+n')+\mathrm{L}\left(\frac{x^2+(n+n')x}{x^2+(n+n')x+nn'}\right)$;

but before we put the third equation into logarithms, it will be better to simplify it; one of the most obvious ways of doing which is to make $n+n'=n''$; then

3d. $\mathrm{L}.x=\mathrm{L}(x+n)+\mathrm{L}(x+n')+\mathrm{L}(x+2n'')-\mathrm{L}(x+n+n'')-\mathrm{L}(x+n'+n'')$

$$+\mathrm{L}\left(\frac{x^3+3n''x^2+(nn'+2n''^2)x}{x^3+3n''x^2+(nn'+2n''^2)x}\right).$$

This may be still farther simplified by making $n=n'$, consequently $n''=2n$, then

$$\mathrm{L}.x=2\mathrm{L}.(x+n)-2\mathrm{L}(x+3n)+\mathrm{L}(x+4n)+\mathrm{L}\left(\frac{x^3+6nx^2+9n^2x}{x^3+6nx^2+9n^2x+4n^3}\right)$$

If now we change n into -1, and x into $x+2$, we shall fall upon the elegant formula of Mr. BORDA.*

* If any one shall attempt to calculate a Table of logarithms by means of differences, each logarithm being got, not from the next, but from the next but one below it, he will fall upon the series of BORDA: for $\Delta^3.\mathrm{L}(x-1)=\mathrm{L}.(x+2)-3\mathrm{L}(x+1)+3\mathrm{L}.x-\mathrm{L}(x-1)$; $\Delta^3.\mathrm{L}(x-2)=\mathrm{L}(x+1)-3\mathrm{L}.x+3\mathrm{L}(x-1)-\mathrm{L}(x-2)$, by adding which, $\Delta^3.\mathrm{L}(x-1)+\Delta^3.\mathrm{L}(x-2)=\mathrm{L}\left(\frac{(x+2)(x-1)^2}{(x-2)(x+1)^2}\right)$ which gives the series we are speaking of. This remark will be exemplified in one of the following Propositions.

In like manner we might investigate approximations of the fourth and following orders: but this kind of research has very little use, and the Proposition was inserted for a different purpose.

PROP. III.

5. *Supposing that, in the last Proposition,* $n=n'=n''=\&c.=-1$. *It is required to find the law of the converging expressions for* L.x.

In this case the four first transformations give

$$x=(x-1)\times\frac{x}{x-1}$$

$$x=(x-1)\times\frac{x-1}{x-2}\times\frac{x(x-2)}{(x-1)^2},$$

$$x=(x-1)\times\frac{x-1}{x-2}\times\frac{(x-1)(x-3)}{(x-2)^2}\times\frac{x(x-2)^3}{(x-1)^3(x-3)}$$

$$x=(x-1)\times\frac{x-1}{x-2}\times\frac{(x-1)(x-3)}{(x-2)^2}\times\frac{(x-1)(x-3)^3}{(x-2)^3(x-4)}\times\frac{x(x-2)^6(x-4)}{(x-1)^4(x-3)^4}$$

which, put into logarithms, give

$$L.x= L(x-1)+L\left(\frac{x}{x-1}\right)$$

$$L.x=2L(x-1)-L(x-2)+L\left(\frac{x(x-2)}{(x-1)^2}\right)$$

$$L.x=3L(x-1)-3L(x-2)+L(x-3)+L\left(\frac{x(x-2)^3}{(x-1)^3(x-3)}\right)$$

$$L.x=4L(x-1)-6L(x-2)+4L(x-3)-L(x-4)+L\left(\frac{x(x-2)^6(x-4)}{(x-1)^4(x-3)^4}\right)$$

where a coincidence may be observed between the coefficients and those in the binomial theorem; and it is easily shown that the same coincidence will have place, how far soever we continue the method; or that, in general, the converging expression will be

$$L.x=\frac{n}{1}L(x-1)-\frac{n(n-1)}{1.2}L(x-2)+\frac{n(n-1)(n-2)}{1.2.3}L(x-3)-\ldots$$

$$\ldots\ldots\ldots+L\left\{\frac{x\times(x-2)^{\frac{n(n-1)}{1.2}}\times(x-4)^{\frac{n(n-1)(n-2)(n-3)}{1.2.3.4}}+\&c.}{(x-1)^{\frac{n}{1}}\times(x-3)^{\frac{n(n-1)(n-2)}{1.2.3}}\times\&c.}\right\}(a)$$

For, if it be denied, let this represent a single result, to pass on to the next, we change x into $x-1$, in the logarithm of the fraction, and add a new logarithm (L) to restore the equality: the equation will thus become

$$\mathrm{L}\,.\,x = \left.\begin{matrix}\frac{n}{1}\\ +1\end{matrix}\right\}\mathrm{L}(x-1)\ \left.\begin{matrix}-\frac{n(n-1)}{1.2}\\ -\frac{n}{1}\end{matrix}\right\}\mathrm{L}(x-2)\ \left.\begin{matrix}+\frac{n(n-1)(n-2)}{1.2.3}\\ +\frac{n(n-1)}{1.2}\end{matrix}\right\}\mathrm{L}(x-3)-\ldots+(\mathrm{L})$$

$$\text{or } \mathrm{L}\,.\,x = \frac{n+1}{1}\mathrm{L}(x-1) - \frac{(n+1)n}{1.2}\mathrm{L}(x-2) + \frac{(n+1)n(n-1)}{1.2.3}\mathrm{L}(x-3) - \ldots + (\mathrm{L}),$$

and, by transposition, we find

$$(\mathrm{L}) = \mathrm{L}\left\{\frac{x \times (x-2)^{\frac{(n+1)n}{1.2}} \times (x-4)^{\frac{(n+1)n(n-1)(n-2)}{1.2.3.4}} \times \&c.}{(x-1)^{\frac{n+1}{1}} \times (x-3)^{\frac{(n+1)n(n-1)}{1.2.3}} \times \&c.}\right\}$$

so that the whole expression is of the same form as before, which is therefore proved to be general.

6. *Cor.* 1. If in the values of x. in the last article, we put for x, in the second $x+1$, in the third $x+2$, in the fourth $x+3$, and so on; and moreover represent the last fractions arising after such substitution by $\alpha, \alpha', \alpha'', \alpha'''$, &c., we get the following set of equations

$$\begin{aligned} x &= (x-1) \times \alpha \\ x+1 &= x \times \alpha \times \alpha' \\ x+2 &= (x+1) \times \frac{x+1}{x} \times \alpha' \times \alpha'' \qquad (b) \\ x+3 &= (x+2) \times \frac{x+2}{x+1} \times \frac{x(x+2)}{(x+1)^2} \times \alpha'' \times \alpha''' \\ &\&c. \ldots\ldots\ldots\ldots \&c. \ldots\ldots \end{aligned}$$

which, *from the manner of their formation*, are subject to this law, that the m^{th} fraction (provided it is not the last) in the value of $x+n$, is equal to $x+n-1$ divided by the product of the first $m-1$ fractions in the expression of $x+n-1$.

If, for brevity, we put L_{1}, L°, L′, L″, &c. for $\mathrm{L}(x-1)$, $\mathrm{L}.x$, $\mathrm{L}(x+1$, $\mathrm{L}(x+2)$, &c. the last equations give

$$
\begin{aligned}
&\mathrm{L}^{\circ} = \mathrm{L}_{1} + \mathrm{L}.\alpha \\
&\mathrm{L}' = \mathrm{L}^{\circ} + \mathrm{L}.\alpha + \mathrm{L}\alpha' \qquad\qquad (c) \\
&\mathrm{L}'' = \mathrm{L}' + (\mathrm{L}' - \mathrm{L}^{\circ}) + \mathrm{L}.\alpha' + \mathrm{L}.\alpha'' \\
&\mathrm{L}''' = \mathrm{L}'' + (\mathrm{L}'' - \mathrm{L}') + (\mathrm{L}'' - 2\mathrm{L}' + \mathrm{L}^{\circ}) + \mathrm{L}.\alpha'' + \mathrm{L}.\alpha''' \\
&\mathrm{L}'''' = \mathrm{L}''' + (\mathrm{L}''' - \mathrm{L}'') + (\mathrm{L}''' - 2\mathrm{L}'' + \mathrm{L}') + (\mathrm{L}''' - 3\mathrm{L}'' + 3\mathrm{L}' - \mathrm{L}^{\circ}) \\
&\qquad\qquad + \mathrm{L}.\alpha''' + \mathrm{L}.\alpha''''
\end{aligned}
$$

. . . .

$$
\begin{aligned}
\mathrm{L}^{''\cdots(n+1)} = {}& \mathrm{L}^{''\cdots n} + (\mathrm{L}^{''\cdots n} - \mathrm{L}^{''\cdots(n-1)}) + (\mathrm{L}^{''\cdots n} - 2\mathrm{L}^{''\cdots(n-1)} + \\
& \mathrm{L}^{''\cdots(n-2)}) + \ldots\ldots + (\mathrm{L}^{''\cdots n} - n\mathrm{L}^{''\cdots(n-1)} + \frac{n(n-1)}{1.2}\mathrm{L}^{''\cdots(n-2)} \\
& - \frac{n(n-1)(n-2)}{1.2.3}\mathrm{L}^{''\cdots(n-3)} +) + \mathrm{L}.\alpha^{''\cdots n} + \mathrm{L}.\alpha^{''\cdots(n+1)}
\end{aligned}
$$

These equations are subject to a law arising from that which we noticed in (*b*), viz. that the m^{th} term (provided it is not the last) in the value of $\mathrm{L}^{''\cdots n}$ is equal to $\mathrm{L}^{''\cdots(n-1)}$, minus the sum of the first $m-1$ terms in the expression $\mathrm{L}^{''\cdots(n-1)}$. By *term* I here mean the whole expression included between two brackets.

If we form $\mathrm{L}\alpha^{''\cdots n}$ generally from the last term of equation (*a*) we have

$$
\mathrm{L}.\alpha^{''\cdots n} = \mathrm{L}\left\{ \frac{(x+n) \times (x+n-2)^{\frac{(n+1)n}{1\,2}} \times (x+n-4)^{\frac{(n+1)n(n-1)(n-2)}{1.2.3.4}} \times \&\text{c.}}{(x+n-1)^{\frac{n+1}{1}} \times (x+n-3)^{\frac{(n+1)n(n-1)}{1.2.3}} \times \&\text{c.}} \right\}
$$

If any one should not be satisfied that the form given to $\mathrm{L}^{''\cdots(n+1)}$ is general, he has only to form $\mathrm{L}^{''\cdots(n+2)}$ from it, and he will find the same form in that case. Now $\mathrm{L}^{''\cdots(n+2)}$ is formed from $\mathrm{L}^{''\cdots(n+1)}$ by changing x into $x+1$ (or $\mathrm{L}^{''\cdots r}$ into $\mathrm{L}^{''\cdots(r+1)}$ in all the terms but the last $\mathrm{L}.\alpha^{''\cdots(n+1)}$, and

this we may remember had x first changed into $x-1$, and afterwards into $x+1$, so that it receives no alteration. Finally there is to be added $L.\alpha''\cdots(n+2)$.

7. *Cor.* 2. Change x into $x-1$ in equations (b), and let the new values of $\alpha, \alpha', \alpha''$, &c. be β, β', β'', &c., and there arises the following series of equations,

$$\begin{aligned}
x-1 &= (x-2)\times\beta \\
x &= (x-1)\times\beta\times\beta' \\
x+1 &= x\times\frac{x}{x-1}\times\beta'\times\beta'' \\
x+2 &= (x+1)\times\frac{x+1}{x}\times\frac{(x-1)(x+1)}{x^2}\times\beta''\times\beta''' \\
&\text{\&c.} \qquad\qquad \text{\&c.}
\end{aligned}$$

which being multiplied by equations (b), the first by the first, the second by the second, and so on, factor by factor, and putting $\alpha\times\beta=s$, $\alpha'\times\beta'=s'$, $\alpha''\times\beta''=s''$, and so on, give

$$\begin{aligned}
x &= (x-2)\times s \\
x+1 &= (x-1)\times s\times s' \\
x+2 &= x\times\frac{x+1}{x-1}\times s'\times s'' \\
x+3 &= (x+1)\times\frac{x+2}{x}\times\frac{(x+2)(x-1)}{(x+1)x}\times s''\times s''' \\
&\text{\&c.} \qquad\qquad \text{\&c.}
\end{aligned}$$

which, being put into logarithms, give

$$\begin{aligned}
L^\circ &= \underset{''}{L} + L.s \\
L' &= \underset{'}{L} + L.s + L.s' \qquad\qquad (d) \\
L'' &= L^\circ + (L' - \underset{'}{L}) + L.s' + L.s'' \\
L''' &= L' + (L'' - L^\circ) + (L'' - L' - L^\circ + \underset{'}{L}) + L.s'' + L.s'''
\end{aligned}$$

. . . .

$L''\cdots(n+1) = L''\cdots(n-1) + (L''\cdots n - L''\cdots(n-2)) + (L''\cdots n - L''\cdots(n-1)$

$-L''^{\dots(n-2)}+L''^{\dots(n-3)})+\dots\dots\dots+(L''^{\dots n}-(n-1)L''^{\dots(n-1)}+$
$\frac{n\times(n-3)}{1.2}L''^{\dots(n-2)}-\frac{n(n-1)\times(n-5)}{1.2.3}L''^{\dots(n-3)}+)+L.s''^{\dots n}+$
$+L.s''^{\dots(n+1)}$; any immediate term, as the $m+1^{th}$, will be of the form $L''^{\dots n}-(m-1)L''^{\dots(n-1)}+\frac{m\times(m-3)}{1.2}L'''^{(n-2)}-\frac{m(m-1)\times(m-5)}{1.2.3}+.$*

We easily see that

$$L.s''^{\dots n}=L\left\{\frac{(x+n)\times(x+n-2)^{\frac{(n+1)\times(n-2)}{1.2}}\times(x+n-4)^{\frac{(n+1)n(n-1)\times(n-6)}{1.2.3.4}}\times\&c.}{(x+n-1)^{n}\times(x+n-3)^{\frac{(n+1)n\times(n-4)}{1.2.3}}\times\&c.}\right\}$$

It is scarcely necessary to say that $L.\alpha''^{\dots n}$, $L.s''^{\dots n}$ are to be expanded by the common series for $L.\frac{a}{b}$. viz.

* These forms are analogous to an expression in the method of differences, which, though not noticed by STIRLING and other writers on interpolation, may be useful on many occasions, as the coefficients are small and few in number. BORDA's expression for logarithms is a particular case of it.

$u_n=(n-1)u_{n-1}-\frac{n\times(n-3)}{1.2}u_{n-2}+\frac{n(n-1)\times(n-5)}{1.2.3}u_{n-3}-\frac{n(n-1)(n-2)\times(n-7)}{1.2.3.4}u_{n-4}$
$+\dots\dots\dots+\Delta^n u+\Delta^n u_{-1}$. If we make $u_n=L.x$, we have, by taking n (in the coefficients) successively 1, 2, 3, 4, &c.

$L.x=L(x-2)+$ series,

$L.x=L(x-1)+L(x-2)-L(x-3)+$ series,

$L.x=2\{L(x-1)-L(x-3)\}+L(x-4)+$ series, (BORDA's if we change x into $x+2$).

$L.x=3\{L(x-1)+L(x-4)\}-2\{L(x-2)+L(x-3)\}-L(x-5)+$series,

$L.x=4\{L(x-1)-L(x-5)\}-5\{L(x-2)-L(x-4)\}+L(x-6)+$series,

$L.x=5\{L(x-1)+L(x-3)+L(x-4)+L(x-6)\}-9\{L(x-2)+L(x-5)\}-L(x-7)+$series,

$L.x=6\{L(x-1)-L(x-7)\}-14\{L(x-2)-L(x-3)+L(x-5)-L(x-6)\}+L(x-8)+$series,

&c. &c.

which may be useful, when taken without the series, as formulas of verification.

$L.\frac{a}{b}=2M\left\{\frac{a-b}{a+b}+\frac{1}{3}\left(\frac{a-b}{a+b}\right)^3+\frac{1}{5}\left(\frac{a-b}{a-b}\right)^5+\&c.\right\}$ though, for the purpose of the two following propositions, they will be better when expanded into series of monomials; see Prop. VI.

Prop. IV.

8. *To construct a Table of Logarithms by means of interpolation from the converging expressions* $L.\alpha$, $L.\alpha'$, $L.\alpha''$, &c.

When treating of the equations marked (c), we noticed a law to which the terms are subject; this law affords an easy method of eliminating the second, third, &c. terms, and, by this means, we find, successively,

$$\begin{array}{ll}
L.x & =L(x-1)+L.\alpha \\
L(x+1) & =L.x+L.\alpha+L.\alpha' \qquad (c) \\
L(x+2) & =L(x+1)+L.\alpha+2L.\alpha'+L.\alpha'' \\
L(x+3) & =L(x+2)+L.\alpha+3L.\alpha'+3L.\alpha''+L.\alpha''' \\
\cdots & \cdots \\
L(x+n) & =L(x+n-1)+L.\alpha+nL.\alpha'+\frac{n(n-1)}{1.2}L.\alpha''+
\end{array}$$

If any one doubts whether this form is general, for every value of n, let it be only a single value; and supposing α, α', α'', &c. to become $\underset{1}{\alpha}$, $\underset{1}{\alpha'}$, $\underset{1}{\alpha''}$, &c., by the substitution of $x+1$ for x, we have

$$L(x+n+1)=L(x+n)+L.\underset{1}{\alpha}+nL.\underset{1}{\alpha'}+\frac{n(n-1)}{1.2}L.\underset{1}{\alpha''}+$$

Now, if we consider the manner in which the last fractional factors, in the values of x, at the beginning of the last proposition, were formed from one another; and the change which they afterwards underwent in forming equations (b), we shall easily perceive that

$\alpha^{''\ldots n} = \dfrac{\alpha_1^{''\ldots(n-1)}}{\alpha^{''\ldots(n-1)}}$; whence $\mathrm{L}.\alpha_1^{''\ldots(n-1)} = \mathrm{L}.\alpha^{''\ldots n} + \mathrm{L}.\alpha^{''(n-1)}$

by means of which equation, $\mathrm{L}(x+n+1)$ becomes

$$\mathrm{L}(x+n+1) = \mathrm{L}(x+n) + \mathrm{L}.\alpha + \frac{n}{1}\mathrm{L}.\alpha' + \frac{n(n-1)}{1.2}\mathrm{L}.\alpha'' + \frac{n(n-1)(n-2)}{1.2.3}\mathrm{L}\alpha''' +$$

$$+ \mathrm{L}.\alpha' + \frac{n}{1}\mathrm{L}\alpha'' + \frac{n(n-1)}{1.2}\mathrm{L}.\alpha''' +$$

or $\mathrm{L}(x+n+1) = \mathrm{L}(x+n) + \mathrm{L}.\alpha + \frac{n+1}{1}\mathrm{L}.\alpha' + \frac{(n+1)n}{1.2}\mathrm{L}.\alpha'' + \frac{(n+1)n(n-1)}{1.2.3}\mathrm{L}.\alpha''' +$ the same form as before, which is therefore general.

9. By means of equations (e) the logarithm of a number is found from the logarithm of that which precedes it, and the process will consist of a number of additions, equal to the number of the values of α we make use of.

Thus, if $\mathrm{L}.\alpha'''$ be so small, as not to affect the last figure, to which we intend to carry the logarithms, it may be neglected and we must make

$$\begin{array}{lll}
\mathrm{L}.\alpha + \mathrm{L}.\alpha' & & = r \\
\mathrm{L}.\alpha + 2\mathrm{L}.\alpha' + \mathrm{L}.\alpha'' = r & + \mathrm{L}.\alpha' + \mathrm{L}.\alpha'' & = r' \\
\mathrm{L}.\alpha + 3\mathrm{L}.\alpha' + 3\mathrm{L}.\alpha'' = r' & + \mathrm{L}\alpha' + 2\mathrm{L}.\alpha'' & = r'' \\
\mathrm{L}.\alpha + 4\mathrm{L}.\alpha' + 6\mathrm{L}.\alpha'' = r'' & + \mathrm{L}.\alpha' + 3\mathrm{L}.\alpha'' & = r''' \\
\ldots & \ldots & \ldots
\end{array}$$

$$\mathrm{L}\alpha + n\mathrm{L}.\alpha' + \frac{n(n-1)}{1.2}\mathrm{L}.\alpha'' = r^{''\ldots(n-2)} + \mathrm{L}.\alpha' + (n-1)\mathrm{L}.\alpha'' = r^{''\ldots(n-1)}$$

Here the quantities r', r'', r''', &c. are formed by two additions each, one more gives the logarithms; for, by substituting in (e),

$$\begin{array}{lll}
\mathrm{L}.x & = \mathrm{L}(x-1) + \mathrm{L}.\alpha & \\
\mathrm{L}(x+1) & = \mathrm{L}.x + \quad \mathrm{L}\alpha + \mathrm{L}\alpha' & = \mathrm{L}.x + r
\end{array}$$

$$L(x+2)=L(x+1)+r+L.\alpha'+L.\alpha''=L(x+1)+r'$$
$$L(x+3)=L(x+2)+r'+L.\alpha'+2L.\alpha''=L(x+2)+r''$$
$$L(x+4)=L(x+3)+r''+L.\alpha'+3L.\alpha''=L(x+3)+r'''$$
&c. &c. &c.

If, in the value of $L.\alpha''^{\cdots n}$ (Art. 6), we put successively 0, 1, 2 for n, we have

$$L.\alpha=L\left(\frac{x}{x-1}\right);\ L.\alpha'=L\left(\frac{(x-1)(x+1)}{x^2}\right);\ L.\alpha''=L\left(\frac{(x+2)x^3}{(x-1)(x+1)^3}\right);$$

or

$$L.\alpha=\ 2M\left\{\frac{1}{2x-1}+\frac{1}{3}\left(\frac{1}{2x-1}\right)^3+\frac{1}{5}\left(\frac{1}{2x-1}\right)^5+\right\}$$
$$L.\alpha'=-2M\left\{\frac{1}{2x^2-1}+\frac{1}{3}\left(\frac{1}{2x^2-1}\right)^3+\frac{1}{5}\left(\frac{1}{2x^2-1}\right)^5+\right\}.$$
$$L.\alpha''=\ 2M\left\{\frac{2x+1}{2x^4+4x^3-2x-1}+\frac{1}{3}\left(\frac{2x+1}{2x^4+4x^3-2x-1}\right)^3+\right\}$$

These are the most converging values, I shall show presently how to expand them into series of monomials.

10. If the intended number of decimal places should require $L.\alpha'''$ also to be retained, make, first

$$L.\alpha+L.\alpha'=r$$
$$L.\alpha+2L.\alpha'+L.\alpha''=r+L.\alpha'+L.\alpha''=r'$$
$$L.\alpha+3L.\alpha'+3L.\alpha''+L.\alpha'''=r'+L.\alpha'+2L.\alpha''+L.\alpha'''=r''$$
$$L.\alpha+4L.\alpha'+6L.\alpha''+4L.\alpha'''=r''+L.\alpha'+3L.\alpha''+3L.\alpha'''=r'''$$

. . . .

$$L.\alpha+nL.\alpha'+\frac{n(n-1)}{1.2}L.\alpha''+\frac{n(n-1)(n-2)}{1.2.3}L.\alpha'''=r''^{\cdots(n-2)}+$$
$$L\alpha'+(n-1)L.\alpha''+\frac{(n-1)(n-2)}{1.2}L.\alpha'''=r''^{\cdot\cdot(n-1)}$$

. Next make

$$L.\alpha'+L.\alpha''=\underset{(1)}{r}$$
$$L.\alpha'+2L.\alpha''+L.\alpha'''=\underset{(1)}{r}+L.\alpha''+L.\alpha'''=\underset{(1)}{r'}$$
$$L.\alpha'+3L.\alpha''+3L.\alpha'''=\underset{(1)}{r'}+L\ \alpha''+2L\alpha'''=\underset{(1)}{r''}$$

$$L.\alpha'+(n-1)L.\alpha''+\frac{(n-1)(n-2)}{1.2}L.\alpha'''=\underset{(1)}{r}{}^{''....(n-3)}+L.\alpha''+$$
$$(n-2)L.\alpha'''\qquad=\underset{(1)}{r}{}^{''....(n-2)}$$

If $L.\alpha''''$ had been used, we must have made $L.\alpha''+L.\alpha''' = \underset{(2)}{r}$, and have proceeded as before.

The substitutions above being made equations (e) become

$$L.x=L(x-1)+L.\alpha$$
$$(Lx+1)=L.x+L\alpha+L\alpha' \qquad =L.x+r=L.x+r$$
$$L(x+2)=L(x+1)+r+L.\alpha'+L.\alpha'' \qquad =L(x+1)+ r'=L(x+1)+r+\underset{(1)}{r}$$
$$L(x+3)=L(x+2)+r'+\underset{(1)}{r}+L.\alpha''+L.\alpha'''=L(x+2)+ r''=L(x+2)+r'+\underset{(1)}{r'}$$
$$L(x+4)=L(x+3)+r''+\underset{(1)}{r'}+L.\alpha''+2L.\alpha'''=L(x+3)+ r'''=L(x+3)+r''+\underset{(1)}{r''}$$
$$L(x+5)=L(x+4)+r'''+\underset{(1)}{r''}+L.\alpha''+3L.\alpha'''=L(x+4)+ r''''=L(x+4)+r'''+\underset{(1)}{r'''}$$

&c. &c.

Where it is plain that each logarithm is found by four additions $\underset{(1)}{r'}$, $\underset{(1)}{r''}$, $\underset{(1)}{r'''}$, &c. being got by two each.

PROP. V.

11. *To construct a Table of Logarithms by means of interpolation from the converging expressions* L . s, L . s′, L . s″, &c.

If we consider the formation of equations (d), we easily perceive that the terms of $L(x+n)$ observe the same law,

with respect to those of $L(x+n-1)$, which we observed in equations (c); we have therefore by a similar elimination,

$$L.x \quad =L(x-2)+L.s$$
$$L(x+1)=L(x-1)+L.s+L.s'$$
$$L(x+2)=L.x \quad +L.s+2L.s'+L.s''$$
$$L(x+3)=L(x+1)+L.s+3L.s'+3L.s''+L.s'''$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot$$
$$L(x+n)=L(x+n-2)+L.s+nL.s'+\tfrac{n(n-1)}{1.2}L.s''+$$

which in order that the logarithms may be got from one another by addition, must be transformed as in the last proposition by the assumption of ρ, ρ', ρ'', &c.; $\underset{(1)}{\rho}$, $\underset{(1)}{\rho'}$, $\underset{(1)}{\rho''}$, &c.; $\underset{(2)}{\rho}$, $\underset{(2)}{\rho'}$ $\underset{(2)}{\rho''}$ &c.: thus if the case only requires us to use $L.s$, $L.s'$, and $L.s''$, make

$$L.s+L.s'=\rho$$
$$L.s+2L.s'+L.s'' \quad =\rho+L.s'+L.s'' \quad =\rho'$$
$$L.s+3L.s'+3L.s'' \quad =\rho'+L.s'+2L.s'' \quad =\rho''$$
$$\&c. \qquad \&c. \qquad \&c.$$

by substituting which our equations become

$$L.x \quad =L(x-2) \quad +L.s$$
$$L(x+1)=L(x-1) \quad +L.s+L.s' \quad =L(x-1)+\rho$$
$$L(x+2)=L.x \quad +\rho+L.s'+L.s'' \quad =L.x \quad +\rho'$$
$$L(x+3)=L(x+1)+\rho'+L.s'+2L.s'' \quad =L(x+1)+\rho''$$
$$\&c. \qquad \&c. \qquad \&c.$$

If now we put successively 0, 1, 2 for n in the value of $L.s''^{\cdots\cdots n}$, given in Art. 7, we find

$$L.s=L\left(\frac{x}{x-2}\right);\ L.s'=L\left(\frac{(x+1)\ (x-2)}{x(x-1)}\right);\ L.s''=L\left(\frac{(x+2)\ (x-1^2}{(x+1)^2(x-2)}\right);\ \text{or}$$

$$L.s= \quad 2M\left\{\frac{1}{x-1}+\frac{1}{3}\left(\frac{1}{x-1}\right)^3+\frac{1}{5}\left(\frac{1}{x-1}\right)^5+\ \&c.\right\}$$

$$L.s' = -2M\left\{\frac{1}{x^2-x-1}+\frac{1}{3}\left(\frac{1}{x^2-x-1}\right)^3+\frac{1}{5}\left(\frac{1}{x^2-x-1}\right)^5+ \&c.\right.$$

$$L.s'' = \quad 2M\left\{\frac{2}{x^3-3x}+\frac{1}{3}\left(\frac{2}{x^3-3x}\right)^3+\frac{1}{5}\left(\frac{2}{x^3-3x}\right)^5+\&c.\right\}$$

(BORDA's Series.)

PROP. VI.

12. *To expand* $L.\alpha''^{\cdots n}$ *and* $L.s''^{\cdots n}$ *into series of monomials of the form* $\frac{A}{x^r}$.

$$L(x+n) = L.x + M\left\{\frac{n}{x}-\frac{n^2}{2x^2}+\frac{n^3}{3x^3}- \ldots\ldots\ldots\ldots \pm\frac{n^r}{rx^r}\mp\right\}$$

$$-\frac{n+1}{1}L(x+n-1) = -\frac{n+1}{1}L.x+M\left\{-\frac{n+1}{1}.\frac{n-1}{x}+\frac{n+1}{1}.\frac{(n-1)^2}{2x^2}\right.$$
$$\left.-\ldots\mp\frac{n+1}{1}.\frac{(n-1)^r}{rx^r}\pm\right\}$$

$$\frac{(n+1)n}{1.2}L(x+n-2) = \frac{(n+1)n}{1.2}L.x+M\left\{\frac{(n+1)n}{1.2}.\frac{n-2}{x}-\frac{(n+1)n}{1.2}.\frac{(n-2)^2}{2x^2}\right.$$
$$\left.+\ldots\pm\frac{(n+1)n}{1.2}.\frac{(n-2)^r}{rx^r}\mp\right\}.$$

&c. &c.

These added together will give $L.\alpha''^{\cdots n}$. It is easy to see that $L.x$ will disappear, because its coefficient $=(1-1)^{n+1}$; we have then, putting Σ to represent the sum of the terms formed by the different values of r,

$$L.\alpha^{\cdots n} = \pm M.\Sigma\left\{\frac{n^r-\frac{n+1}{1}(n-1)^r+\frac{(n+1)n}{1.2}(n-2)^r-\frac{(n+1)n(n-1)}{1.2.3}(n-3)^r+}{rx^r}\right\}$$

where for r we are to take every whole number from one upwards; thus

$$L.\alpha = \quad M\left\{\frac{1}{x}+\frac{1}{2x^2}+\frac{1}{3x^3}+\ldots\ldots\ldots\ldots+\frac{1}{rx^r}+\right\}$$

$$L.\alpha' = -2M\left\{\frac{1}{x^2}+\frac{1}{2x^4}+\frac{1}{3x^6}+\&c.\right\}$$

&c. &c.

Hence $L.\alpha'$ equals double the sum of the second, fourth, &c.

terms of $L.\alpha$, with the sign changed. $L.s^{\prime\prime\cdots n}$ is expanded, by means of the logarithmic series, in a similar manner.

Prop. VII.

13. *To calculate a Table of Logarithmic sines, or cosines.*

It is quite evident, that, if, in the fractional products, in Propositions II, and III, instead of x, $x-1$, $x-2$, &c. we had used successively cos. x, cos. $(x-u)$, cos. $(x-2u)$, &c. or sin. x, sin. $(x-u$ sin. $(x-2u)$, &c., the reasonings made use of would have been equally applicable; and that the whole methods given in Proposition III, IV, V, including the general expressions for $L.\alpha^{\prime\prime\cdots n}$, $Ls^{\prime\prime\cdots n}$, (but not the expansions of the said expressions) will hold good here, after we have made the above mentioned substitutions. Thus if it is L. cos. x which we are calculating, we shall have

$$L.\alpha = L\left(\frac{\cos. x}{\cos. (x-u)}\right);\ L.\alpha' = L.\left(\frac{\cos. (x-u)\cos. (x+u)}{\cos.^2 x}\right);\ L.\alpha'' = L\left(\frac{\cos. (x+2u)\cos.^3 x}{\cos. (x-u)\cos.^3(x+u)}\right)$$

These are the logarithms of numbers converging continually towards unity, and must be found by the form for $L.\left(\frac{a}{b}\right)$.

XVI. *Two general propositions in the method of differences.* *By* Thomas Knight, *Esq.* *Communicated by* Taylor Combe, *Esq. Sec. R. S.*

Read February 27, 1817.

1. THOUGH so many ingenious writers have demonstrated, and, in various respects, extended the celebrated formulas of LA GRANGE, for $\Delta^n\varphi(x)$, $\Sigma^n\varphi(x)$ no one appears to have entertained the idea, that these, and the more general cases, in which the quantities under the functional sign have their differences variable, might be included in one simple form.

Mr. PRONY* is, I believe, the only mathematician who has given a form of any regularity to $\Delta^n\varphi(x)$, when the difference of x is variable; but he does not seem to have been aware of the capability of the method he was employing; and instead of embracing, as he might have done, all cases in one simple expression, he has proposed a formula which has neither any particular elegance in itself, nor any apparent relation to that which, in the simpler case, had been given by LA GRANGE.

I suppose the truth of the differential equations

$$\Delta^n\varphi=\varphi_n-\frac{n}{1}\varphi_{n-1}+\frac{n(n-1)}{1.2}\varphi_{n-2}-\frac{n(n-1)(n-2)}{1.2.3}\varphi_{n-3}+, \quad (1)$$

$$\Sigma\varphi=\varphi_{-n}+\frac{n}{1}\varphi_{-(n+1)}+\frac{n(n+1)}{1.2}\varphi_{-(n+2)}+\frac{n(n+1)(n+2)}{1.2.3}\varphi_{-(n+3)}+,\ldots(2):$$

where φ is any variable function whatever.

* LACROIX "Calc. des Diff." p. 25.

PROP. I.

2. *To find the* n^{th} *difference of a function of any number of variable quantities,* $\Delta^n \varphi(x, y, z, \&c.)$, *when the differences of* x, y, z, &c. *are any how variable.*

We will begin with a function of two variables;

Let $x_1 - x = u_1,\ x_2 - x = u_2,\ x_3 - x = u_3, \ldots\ldots x_n - x = u_n;$
$y_1 - y = w_1,\ y_2 - y = w_2,\ y_3 - y = w_3, \ldots\ldots y_n - y = w_n;$ } these values are simultaneous.

Let $\left(\frac{d^n\varphi(x,y)}{dx^n}\right) + \left(\frac{d^n\varphi(x,y)}{dx^{n-1}dy}\right) + \left(\frac{d^n\varphi(x,y)}{dx^{n-2}dy^2}\right) + \ldots\ldots + \left(\frac{d^n\varphi(x,y)}{dy^n}\right)$

be represented by $\Sigma\left(\frac{d^n\varphi(x,y)}{dx^{n-m}dy^m}\right)$; the sign Σ expressing *here* the sum of all the different values that will arise to the function within the brackets, by giving successively to m the values 0, 1, 2, 3, ... n

Lastly, let the symbol ⊠ represent what may be called elective multiplication; thus $\Sigma\left(\frac{d^n\varphi(x,y)}{dx^{n-m}dy^m}\right) \boxtimes (u+w)^n$ will denote that each value of $\left(\frac{d^n\varphi(x,y)}{dx^{n-m}dy^m}\right)$ is to be multiplied by the corresponding term of the expanded binomial $(u+w)^n$; viz. $\left(\frac{d^n\varphi(x,y)}{dx^n}\right)$ by u^n, $\left(\frac{d^n\varphi(x,y)}{dx^{n-1}dy}\right)$ by $nu^{n-1}w$, $\left(\frac{d^n\varphi(x,y)}{dx^{n-2}dy^2}\right)$ by $\frac{n(n-1)}{1.2}u^{n-2}w^2$, and so on. Then

$$\varphi(x+u_1, y+w_1) = \varphi(x,y) + \Sigma\left(\frac{d\varphi(x,y)}{dx^{1-m}dy^m}\right) \boxtimes (u_1+w_1) + \frac{1}{2}\,.$$
$$\Sigma\left(\frac{d^2\varphi(x,y)}{dx^{2-m}dy^m}\right) \boxtimes (u_1+w_1)^2 + \frac{1}{2.3}\,.\,\Sigma\,.\left(\frac{d^3\varphi(x,y)}{dx^{3-m}dy^m}\right) \boxtimes (u_1+w_1)^3 +$$

$$\varphi(x+u_2, y+w_2)=\varphi(x,y)+\Sigma\left(\frac{d\varphi(x,y)}{dx^{1-m}dy^m}\right)\boxtimes(u_2+w_2)+\frac{1}{2}\cdot\Sigma\left(\frac{d^2\varphi(x,y)}{dx^{2-m}dy^m}\right)\boxtimes(u_2+w_2)^2+\frac{1}{2.3}\cdot\Sigma\left(\frac{d^3\varphi(x,y)}{dx^{3-m}dy^m}\right)\boxtimes(u_2+w_2)^3+$$

$$\varphi(x+u_3, y+w_3)=\varphi(x,y)+\Sigma\left(\frac{d\varphi(x,y)}{dx^{1-m}dy^m}\right)\boxtimes(u_3+w_3)+\frac{1}{2}\cdot\Sigma\left(\frac{d^2\varphi(x,y)}{dx^{2-m}dy^m}\right)\boxtimes(u_3+w_3)^2+\frac{1}{2.3}\cdot\Sigma\left(\frac{d^3\varphi(x,y)}{dx^{3-m}dy^m}\right)\boxtimes(u_3+w_3)^3+$$

&c. &c.

These values substituted in the equation $\Delta^n\varphi=\varphi_n-\frac{n}{1}\cdot\varphi_{n-1}+\frac{n(n-1)}{1.2}\cdot\varphi_{n-2}-\frac{n(n-1)(n-1)}{1.2.3}\cdot\varphi_{n-3}+$ give

$$\begin{aligned}\Delta^n\varphi(x,y)=\varphi(x,y)&\left\{1-\frac{n}{1}+\frac{n(n-1)}{1.2}-\frac{n(n-1)(n-2)}{1.2.3}+\right\}\\ &+\Sigma\left(\frac{d\varphi(x,y)}{dx^{1-m}dy^m}\right)\boxtimes\left\{(u_n+w_n)-\frac{n}{1}(u_{n-1}+w_{n-1})\right.\\ &\qquad\left.+\frac{n(n-1)}{1.2}(u_{n-2}+w_{n-2})-\frac{n(n-1)(n-2)}{1.2.3}(u_{n-3}+w_{n-3})+\right\}\\ &+\frac{1}{2}\Sigma\left(\frac{d^2\varphi(x,y}{dx^{2-m}dy^m}\right)\boxtimes\left\{(u_n+w_n)^2-\frac{n}{1}(u_{n-1}+w_{n-1})^2\right.\\ &\qquad\left.+\frac{n(n-1)}{1.2}(u_{n-2}+w_{n-2})^2-\frac{n(n-1)(n-2)}{1.2.3}(u_{n-3}+w_{n-3})^2+\right\}\\ &+\cdot\frac{1}{2.3}\Sigma\left(\frac{d^3\varphi(x,y)}{dx^{3-m}dy^m}\right)\boxtimes\left\{(u_n+w_n)^3-\frac{n}{1}(u_{n-1}+w_{n-1})^3\right.\\ &\qquad\left.+\frac{n(n-1)}{1.2}(u_{n-2}+w_{n-2})^3-\frac{n(n-1)(n-2)}{1.2.3}(u_{n-3}+w_{n-3})^3+\right\}\end{aligned}$$

&c. &c.

3. Now, by the general formula for the n^{th} difference of a variable quantity, (1),

$\Delta^n(x+y)^m=(x+y+u_n+w_n)^m-\frac{n}{1}(x+y+u_{n-1}+w_{n-1})^m+\frac{n(n-1)}{1.2}(x+y+u_{n-2}+w_{n-2})^m-$ whence, making x and y vanish,

$$\Delta^n(o+o')^m = (u_n + w_n)^m - \frac{n}{1}(u_{n-1} + w_{n-1})^m + \frac{n(n-1)}{1.2}(u_{n-2} + w^{-2})^m - ;$$

the successive values of o being $u_1, u_2, u_3, \ldots\ldots u_n$

those of o' being $w_1, w_2, u_3, \ldots\ldots w_n$

observing therefore that $1 - \frac{n}{1} + \frac{n(n-1)}{1.2} - \frac{n(n-1)(n-2)}{1.2.3} +$

$= \Delta^n(o+o')^o$, and putting $\Sigma\left(\frac{d^o\varphi(x,y)}{dx^{o-m}dy^m}\right)$ for $\varphi(x,y)$ our equation takes this form

$$\Delta^n\varphi(x,y) = \Sigma\left(\frac{d^o\varphi(x,y)}{dx^{o-m}dy^m}\right) \boxtimes \Delta^n(o+o')^o + \Sigma\left(\frac{d^1\varphi(x,y)}{dx^{1-m}dy^m}\right) \boxtimes \Delta^n(o+o')^1 +$$

$$\frac{1}{2}\Sigma\left(\frac{d^2\varphi(x,y)}{dx^{2-m}dy^m}\right) \boxtimes \Delta^n(o+o')^2 + \frac{1}{2.3}\,.\,\Sigma\left(\frac{d^3\varphi(x,y)}{dx^{3-m}dy^m}\right) \boxtimes \Delta^n(o+o')^3 + .$$

What has been done with respect to a function of two variables, the analyst will immediately see how to extend to a function containing any number; we may therefore without entering into any farther particulars, give the following

General Rule.

Let the successive values

of o be $u_1, u_2, u_3, \ldots\ldots u_n$

of o' be $w_1, w_2, w_3, \ldots\ldots w_n$

of o'' be $v_1, v_2, v_3, \ldots\ldots v_n$ *

... &c. .... ,*then will*

$$\Delta^n\varphi(x,y,z,\&c.) = \Delta^n e^{o+o'+o''+\&c.} \qquad (3)$$

provided that, after the expansion, we multiply every where a term of the form $A \times u^a{}_{n-r} \times w^b{}_{n-r} \times v^c{}_{n-r} \times$ &c. *by*

* Supposing $z_n - z = v_n$, &c. &c.

$$\left(\frac{d^{a+b+c+\&c.}\,\varphi(x, y, z, \&c.)}{dx^a . dy^b . dz^c . \&c.}\right).$$

4. The expression of LA GRANGE is a particular case of eq. (3), to perceive which we must observe that

$$\Delta^n e^{x+y+z+\&c.} = e^{x+y+z+\&c.+u_n+w_n+v_n+\&c.}$$
$$-\frac{n}{1} . e^{x+y+z+\&c.+u_{n-1}+w_{n-1}+v_{n-1}+\&c.} +$$
$$\frac{n(n-1)}{1.2} e^{x+y+z+\&c.+u_{n-2}+w_{n-2}+v_{n-2}+\&c.} + \&c.$$ whence

$$\Delta^n e^{o+o'+o''+\&c.} = e^{u_n+w_n+v_n+\&c.} - \frac{n}{1} e^{u_{n-1}+w_{n-1}+v_{n-1}} + \&c.$$
$$+\frac{n(n-1)}{1.2} . e^{u_{n-2}+w_{n-2}+v_{n-2}+\&c.} - \&c.;$$

which, if x, y, z, &c. have constant differences, or if u^n, u^{n-1}, u_{n-2}, &c. w_n, w_{n-1}, w_{n-2}, &c. v_n, v_{n-1}, v_{n-1} &c., &c. are nu, $(n-1)u$, $(n-2)u$, &c., nw, $(n-1)w$, $(n-2)w$, &c., nv, $(n-1)v$, $(n-2)v$, &c. &c. becomes

$$\left\{e^{u+w+v+\&c.}-1\right\}^n.$$

The equation (3) may be presented under another form; for if we compare the values of

$\Delta^n e^{x+y+z+\&c.}$ and $\Delta^n e^{o+o'+o''+\&c.}$ we see that

$$\Delta^n e^{o+o'+o''+\&c.} = \frac{\Delta^n e^{x+y+z+\&c.}}{e^{x+y+z+\&c.}},$$ consequently

$$\Delta^n\varphi(x, y, z, \&c.) = \frac{\Delta^n e^{x+y+z+\&c.}}{e^{x+y+z+\&c.}} \qquad (4)$$

where we must observe, with respect to the differential coefficients, the same rule as was given with eq. (3).

PROP. II.

5. *To find* $\Sigma^n \varphi(x, y, z, \&c.)$ *supposing that the differences of* x, y, z, &c. *are any how variable.*

We shall here make use of the same notation as we did in Prop. I, only let the preceding values

of x be $x-u_{-1}$, $x-u_{-2}$, $x-u_{-3}$, &c.
of y be $y-w_{-1}$, $y-w_{-2}$, $y-w_{-3}$, &c.
of z be $z-v_{-1}$, $z-v_{-2}$, $z-v_{-3}$, &c.
&c. &c.

It will be sufficient also to consider the case of two variable quantities, as was done in Prop. I.

First, we have, in general,

$$\varphi(x-u_{-r}, y-w_{-r}) = \varphi(x, y) - \Sigma\left(\frac{d\varphi(x, y)}{dx^{1-m}dy^m}\right) \boxtimes (u_{-r}+w_{-r}) +$$

$$+ \frac{1}{2} \cdot \Sigma\left(\frac{d^2\varphi(x, y)}{dx^{2-m}dy^m}\right) \boxtimes (u_{-r}+w_{-r})^2 - \frac{1}{2.3} \cdot \Sigma\left(\frac{d^3\varphi(x, y)}{dx^{3-m}dy^m}\right)$$

$\boxtimes (u_{-r}+w_{-r})^3 +$ which expression being combined with (2), putting for the sake of symmetry $\varphi(x, y) = \Sigma\left(\frac{d^0\varphi(x, y)}{dx^{0-m}dy^m}\right)$, gives

$$\Sigma^n \varphi(x, y) = \Sigma\left(\frac{d^0\varphi(x, y)}{dx^{0-m}dy^m}\right) \boxtimes \Big\{(u_{-n}+w_{-n})^0 + \frac{n}{1} \cdot (u_{-(n+1)}$$

$$+ w_{-(u+1)})^0 + \frac{n(n+1)}{1.2}(u_{-(n+2)}+w_{-(n+2)})^0 + \&c.\Big\}$$

$$- \Sigma\left(\frac{d^1\varphi(x, y)}{dx^{1-m}dy^m}\right) \boxtimes \Big\{(u_{-n}+w_{-n})^1 + \frac{n}{1} \cdot (u_{-(n+1)}$$

$$+ w_{-(n+1)})^1 + \frac{n(n+1)}{1.2}(u_{-(n+2)}+w_{-(n+2)})^1 + \&c.\Big\}$$

$$+\frac{1}{2}\cdot\Sigma\left(\frac{d^2\varphi(x,y)}{dx^{2-m}dy^m}\right)\boxtimes\left\{(u_{-n}+w_{-n})^2+\frac{n}{1}\cdot(u_{-(n+1)}+w_{-(n+1)})^2\right.$$
$$\left.+\frac{n(n+1)}{1.2}(u_{-(n+2)}+w_{-(n+2)})^2+\&c.\right\}$$
$+$ &c.

But by equat. (2) $\Sigma^n(x+y)^m=(x+y-u_{-n}-w_{-n})^m+\frac{n}{1}\cdot$
$(x+y-u_{-(n+1)}-w_{-(n+1)})^m+\frac{n(n+1)}{1.2}(x+y-u_{-(n+2)}-w_{-(n+2)})^m+$
whence

$$\Sigma^n(o+o')^m=\pm\left\{(u_{-n}+w_{-n})^m+\frac{n}{1}(u_{-(n+1)}+w_{-(n+1)})^m+\right.$$
$$\left.\frac{n(n+1)}{1.2}\cdot(u_{-(n+2)}+w_{-(n+2)})^m+\right\}$$

The upper or lower sign having place accordingly as m is even or odd. So that our equation may be expressed thus,

$$\Sigma^n\varphi(x,y)=\Sigma\left(\frac{d^0\varphi(x,y)}{dx^{0-m}dy^m}\right)\boxtimes\Sigma^n(o+o')^0+\Sigma\left(\frac{d^1\varphi(x,y)}{dx^{1-m}dy^m}\right)\boxtimes\Sigma^n(o+o')^1+$$
$$\frac{1}{2}\cdot\Sigma\left(\frac{d^2\varphi(x,y)}{dx^{2-m}dy^m}\right)\boxtimes\Sigma^n(o+o')^2+\frac{1}{2.3}\cdot\Sigma\left(\frac{d^3\varphi(x,y)}{dx^{3-m}dy^m}\right)\boxtimes\Sigma^n(o+o')^3+$$

and the analyst will, without any trouble, see the truth of the following rule for a function of any number of variables.

Let the preceding values

of o be $-u_{-1}, -u_{-2}, -u_{-3}, \ldots\ldots -u_{-n}$, &c.
of o' be $-w_{-1}, -w_{-2}, -w_{-3}, \ldots\ldots -w_{-n}$, &c.
of o'' be $-v_{-1}, -v_{-2}, -v_{-3}, \ldots\ldots -v_{-n}$, &c.
&c. &c.

then will

$$\Sigma^n\varphi(x,y,z,\&c.)=\Sigma^n e^{o+o'+o''+\&c.} \qquad (5)$$

provided that, after expansion, we multiply a term of the form
$A\times u^a_{-(n+r)}\times w^b_{-(n+r)}\times v^c_{-(n+r)}\times$ &c. *by*

$$\left(\frac{d^{a+b+c+\&c.}\varphi(x,y,z,\&c.)}{dx^a\times dy^b\times dz^c\times\&c.}\right)$$

We have by form (2),

$$\Sigma^n e^{x+y+\ +\&c.} = e^{x+y+z+\&c.-u_{-n}-w_{-n}-v_{-n}-\&c.} +$$
$$\frac{n}{1} \cdot e^{x+y+z+\&c.-u_{-(n+1)}-w_{-(n+1)}-v_{-(n+1)}-\&c.} +$$
$$\frac{n(n+1)}{1.2} \cdot e^{x+y+z+\&c.-u_{-(n+2)}-w_{-(n+2)}-v_{-(n+2)}-\&c.} +$$

&c. . . . . so that

$$\begin{aligned} \Sigma^n e^{o+o'+o''+\&c.} &= e^{-u_{-n}-w_{-n}-v_{-n}-\&c.} + \\ &\frac{n}{1} \cdot e^{-u_{-(n+1)}-w_{-(n+1)}-v_{-(n+1)}-\&c.} + \qquad (6) \\ &\frac{n(n+1)}{1.2} \cdot e^{-u_{-(n+2)}-w_{-(n+2)}-v_{-(n+2)}-\&c.} + \&c. \end{aligned}$$

By comparing these expressions, it appears that

$\Sigma^n e^{o+o'+o''+\&c.} = \frac{\Sigma^n e^{x+y+z+\&c.}}{e^{x+y+z+\&c.}}$ and consequently, that

$\Sigma^n \phi(x, y, z, \&c.) = \frac{\Sigma^n . e^{x+y+z+\&c.}}{e^{x+y+z+\&c.}}$, provided, &c.

It is scarcely necessary for me to observe, that the second member of the equation marked (6) becomes $\left\{ e^{u+w+v+\&c.} - 1 \right\}^{-n}$ in the case of constant differences of x, y, z, &c.; for u_{-n}, $u_{-(n+1)}$, &c. become in this case, nu, $(n+1)\ n$, &c., and the w, *s* and v, *s* undergo a similar change.

The results of the preceding propositions may be brought into a very small compass, viz.

Δ^{-n} *representing* Σ^n, *the* nth *difference or the* nth *integral of a function of any number of variable quantities, and varying in any possible manner, will be expressed by the equation* $\Delta^n \phi(x, y, z, \&c.) = \frac{\Delta^n e^{x+y+z+\&c.}}{e^{x+y+z+\&c.}}$; *provided that after expansion, we multiply*

&c. &c. &c.

6. SCHOLIUM.

We may find, in many cases, very elegant and regular expressions for $\Delta^n\varphi(x)$, by supposing $\varphi(x+u)$ to be expanded differently from the form given by TAYLOR'S theorem: as, for instance,

If $\varphi(x+u)=\psi(x)+X.\chi(u)+X'\chi'(u)+X''\chi''(u)+$, (7) *where* X, X', X'', &c. *represent any functions whatever of* x, and φ, ψ, χ, χ' &c. *any functions of the quantities they stand before, then* Δx, or u, *being constant,*

$\Delta^n\varphi(x)=X.\Delta^n.\chi(o)+X'.\Delta^n.\chi'(o)+X''.\Delta^n.\chi''(o)+$&c.: for

$$\varphi(x+nu)=\psi(x)+X.\chi(nu)+X'.\chi'(nu)+$$

$$-\frac{n}{1}.\varphi(x+\overline{n-1}.u)=-\frac{n}{1}.\psi(x)-\frac{n}{1}X.\chi(\overline{n-1}.u)-\frac{n}{1}.X'.\chi'(\overline{n-1}.u)-$$

$$+\frac{n(n-1)}{1.2}\varphi(x+\overline{n-2}.u)=\frac{n(n-1)}{1.2}\psi(x)+\frac{n(n-1)}{1.2}.X.\chi(\overline{n-2}.u)+\frac{n(n-1)}{1.2}.X'.\chi'(\overline{n-2}).u)+$$

&c. &c.

and because $\Delta^n.\varphi=\varphi_n-\frac{n}{1}\varphi_{n-1}+\frac{n(n-2)}{1.2}\varphi_{n-2}-$, this being added give

$$\Delta^n.\varphi(x)=X.\Delta^n.\chi(o)+X'.\Delta^n.\chi'(o)+X''.\Delta^n.\chi''(o)+ \quad (8)$$

If form (7) soon terminates, the expressions for the differences are very simple, as in

Ex. 1. Sin. $(x+u)=$ sin. x cos. $u+$ cos. x, sin. u, which being compared with (7) gives X $=$ sin. x, X' $=$ cos. x, X'', &c.$=o$; $\chi(o)=$ cos. o, $\chi'(o)=$ sin. o, $\chi''(o)$, &c. $=o$; whence

$$\Delta^n.\text{ sin. } x=\text{sin. } x.\ \Delta^n.\text{ cos. } o+\text{cos. } x.\ \Delta^n.\text{ sin. } o.$$

Ex. 2. Tang. $(x+u)=$tang. $x+$ sec.$^2x\{$tang. $u+$tang. x.

tang.$^2 u$ + tang.$^2 x$. tang.$^3 u + \}$, see DELAMBRE, Preface to BORDA, p. 48, whence, by our expression,

$$\Delta^n. \text{tang.}\, x = \sec.^2 x \{ \Delta^n \text{tang.}\, o + \text{tang.}\, x\,.\, \Delta^n.\, \text{tang.}^2 o + \text{tang.}^2 x.\, \Delta^n.\, \text{tang.}^3 o + \}$$

Ex. 3. L. sin. $(x + u)$ = L. sin. x + L. cos. u + M $\{$ Cot. x. tang. $u - \frac{1}{2}$ cot.$^2 x$. tang.$^2 u + \frac{1}{3}$ cot.$^3 x$ tang.$^3 u + \}$ DELAMBRE, Preface to BORDA, p. 45; comparing with (7) and (8) we find Δ^n. L. sin. $x = \Delta^n$. L. cos. o +

$$M \{ \text{Cot.}\, x \Delta^n.\text{tang.}\, o - \tfrac{1}{2} \text{cot}^2. x \Delta^n. \text{tang.}^2 o + \tfrac{1}{3} \text{cot.}^3 x. \Delta^n. \text{tang.}^3 o - \}$$

In like manner, because

$$\text{L.}\cos.(x+u) = \text{L.}\cos.\, x + \text{L.}\cos.\, u - M \{ \text{tang.}\, x.\, \text{tang.}\, u + \tfrac{1}{2}\, \text{tang.}^2 x.\, \text{tang.}^2 u + \tfrac{1}{3}\, \text{tang.}^3 x.\, \text{tang.}^3 u + \}$$

$$\Delta^n.\text{L.}\cos.\, x = \Delta^n.\, \text{L.}\cos.\, o - M \{ \text{tang.}\, x\, \Delta^n.\, \text{tang.}\, o + \tfrac{1}{2}\, \text{tang.}^2 x\, \Delta^n.\, \text{tang.}^2 o + \tfrac{1}{3}\, \text{tang.}^3 x\, \Delta^n\, \text{tang.}^3 o + \}$$

Then, because L. tang. = L. sin. — L. cos., Δ^n. L. tang. = Δ^n. L. sin. — Δ^n. L. cos., therefore

$$\Delta^n.\text{L.}\,\text{tang.}\, x = M \{ (\text{Cot.}\ x + \text{tang.}\ x)\, \Delta^n.\, \text{tang.}\, o - \tfrac{1}{2} (\text{cot.}^2 x - \text{tang.}^2 x)\, \Delta^n.\, \text{tang.}^2 o + \tfrac{1}{3} (\text{cot.}^3 x + \text{tang.}^3 x)\, \Delta^n.\, \text{tang.}^3 o - \}$$

If these forms were to be used for interpolation, we should have to calculate, before the commencement of a Table, Δ L. cos. o, Δ^2. L. cos. o, &c.; Δ. tang. o, Δ^2. tang. o, &c.; Δ. tang.$^2 o$, Δ^2. tang.$^2 o$, &c., &c. These latter quantities are to be multiplied by M, and will then serve for calculating the whole Table.

If three differences are sufficient, we have, making $u = 1'$,*

* It is the decimal division of the circle which is supposed here.

$\Delta . L . \cos. o = -,0^{8}53579$, $\Delta^{2} . L . \cos. o = -,0^{7}107158$, $\Delta^{3} . L . \cos. o =,0^{13}$;

$\Delta . \text{tang}. o =,0^{3}1570796339$, $\Delta^{2} . \text{tang}. o =,0''78$, $\Delta^{3}. \text{tang}. o =,0''78$;

$\Delta . \text{tang}.^{2} o =,0^{7}246740$, $\Delta^{2}. \text{tang}.^{2} o =,0^{7}493480$, $\Delta^{3}. \text{tang}.^{2} o = o,0^{13}$;

$\Delta . \text{tang}.^{3} o =,0''39$, $\Delta.^{2} \text{tang}.^{3} o =,0^{10}232$, $\Delta^{3}. \text{tang}.^{3} o =,0^{10}232$.

Suppose, for a particular example, we want the first three differences of L. sin. 50°, we have

$$\Delta L . \sin. 50^{\circ} = -,0^{8}53579 + M\{,0^{3}1570796339 -,0^{7}123370 +,0''13\}$$

$$\Delta^{2} L . \sin. 50^{\circ} = -,0^{7}107158 + M\{,0''78 -,0^{7}246740 +,0''77\}$$

$$\Delta^{3}. L . \sin. 50^{\circ} = M\{,0''78 +,0''77\} \text{ or}$$

$\Delta \; L . \sin. 50^{\circ} =,0000682081030$, $\Delta^{2} . L . \sin . 50^{\circ} = -,000000214249$,

$\Delta^{3}. L . \sin. 50^{\circ} =,00000000000068$.

XVII. *Note respecting the demonstration of the binomial theorem inserted in the last volume of the Philosophical Transactions. By* Thomas Knight, *Esq. Communicated by* Taylor Combe, *Esq. Sec. R. S.*

Read April 17, 1817.

In looking into Mr. Spence's ingenious "Essay on Logarithmic Transcendents," a work published in 1809, but which I have been so unfortunate as never to have seen till within the last fortnight, I was not a little surprised to find that a demonstration of the binomial theorem, similar to the one I had the honour to present to the Royal Society, had been already given by that writer. The same may be said of the first proposition of the preceding Paper on the construction of Logarithms.

Having made this acknowledgment, I shall perhaps be pardoned for observing, that Mr. Spence is not particularly happy in the manner of developing the kind of functions he treats of in his preface. I shall endeavour to give the solution of a class of equations of which he (Pref. p. vii.) has considered a particular case: with this we will begin.

It is proposed to develope the function which has this property, viz.

$$\varphi(1+x)+\varphi(1+y)+\varphi(1+z)=\varphi(1+x.1+y)+\varphi(1+x.1+z)+\varphi(1+y.1+z)-\varphi(1+x.1+y.1+z).$$

Assume $\varphi(1+x)=A+A'x+A''x^2+A'''x^3+\ldots\ldots A^{''\ldots\ldots n}x^n+$, and after making the requisite substitutions in the given

equation, we see immediately that $A = o$, and that A' and A'' are arbitrary. Then, to find the law of the coefficients, in this and other similar cases, where there are any number of independent quantities, $x, x', x'', \ldots\ldots x''^{\cdots n}$, transpose all the equation to one side, and find the coefficient of the first power of $x''^{\cdots n}$, then the coefficient of the first power of $x''^{\cdots n-1}$ in the former coefficient, then again, in this last, the coefficient of the first power of $x''^{\cdots n-2}$: and having arrived in this manner at the coefficient of x', it will have the form $a + bx + cx^2 + \ldots\ldots + rx^n +$, and the equation $r = o$ will give the law sought for.

In the present case, putting $\rho = x + (1 + x)z$, $\sigma = 1 + x + (1 + x)z$, we find, by equalling to o the coefficient of the first power of y,

$$o = \left.\begin{matrix} A' + 2A'' \\ + A' \end{matrix}\right\} (x + z) \left.\begin{matrix} +3A''' \\ +2A'' \end{matrix}\right\} (x^2 + z^2) \left.\begin{matrix} + \ldots\ldots + (n+1)A''^{\cdots(n+1)} \\ + \ldots\ldots + \quad nA''^{\cdots n} \end{matrix}\right\} (x^n + z^n) \begin{matrix} + \\ + \end{matrix}$$

$$- A'\sigma - 2A''\sigma\rho - 3A'''\sigma\rho^2 - \ldots\ldots - (n + 1(A''^{\cdots(n+1)}\sigma\rho^n -$$

If in this we equal to o the coefficient of the first power of z, there arises

$$\begin{aligned} o = 2A'' - A'x - 2A''\ & (1 + 3x + 2x^2) \\ - 3A'''\ & (2x + 5x^2 + 3x^3) \\ - 4A''''\ & (3x^2 + 7x^3 + 4x^4) \\ & \ldots\ldots\ldots \\ - nA''^{\cdots n}\ & (\overline{n-1} \,.\, x^{n-2} + \overline{2n-1} \,.\, x^{n-1} + nx^n) \\ - \quad & \&c. \ldots\ldots\ldots \end{aligned}$$

whence we find for the general law of the coefficients $(n > 2)$,

$$n(n-1)A''^{\cdots n} + \overline{2n-3} \,.\, \overline{n-1} \,.\, A''^{\cdots(n-1)} + (n-2)^2 A''^{\cdots(n-2)} = o \ldots\ldots (1)$$

From which let us suppose that we have calculated a few of the coefficients, and arrived at the result of Mr. SPENCE, viz.

$\varphi(1+x)=A'(x-\frac{x^3}{2.3}+\frac{5x^4}{2.3.4}-)+A''L^2(1+x)\ldots\ldots(2)$

nothing can be easier than to find the value of the remaining series; for it is quite obvious, from the equation expressing the property of the function, that $L(1+x)$ is a particular value of $\varphi(1+x)$. The same also appears from equation (1), in which, if we put $A''^{\ldots(n-2)}=\frac{1}{n-2}$, $A''^{\ldots(n-1)}=\frac{-1}{n-1}$, $A'^{\ldots n}=\frac{1}{n}$, the left hand member vanishes.

Make then, in equation (2), $A'=1$, $A''=\frac{-1}{2}$, and it becomes

$L(1+x)=x-\frac{x^3}{2.3}+\frac{5x^4}{2.3.4}-\&c.-\frac{1}{2}L^2(1+x)$, whence

$x-\frac{x^3}{2.3}+\frac{5x^4}{2.3.4}-\&c.=L(1+x)+\frac{1}{2}L^2(1+x)$, and finally

$\varphi(1+x)=A'\left\{L(1+x)+\frac{1}{2}L^2(1+x)\right\}+A''L^2(1+x)$

Let us now endeavour to develope the function next in order of the same class, viz. $\varphi(1+x)$, having the following property,

$$\begin{aligned}\varphi(1+w)+\varphi(1+x)+\varphi(1+y)+\varphi(1+z)=\varphi(1+x\,.\,1+y)\\+\varphi(1+x\,.\,1+z)+\\\varphi(1+y\,.\,1+z)+\varphi(1+w\,.\,1+x)+\varphi(1+w\,.\,1+y)+\\\varphi(1+w\,.\,1+z)-\ldots\ldots(3)\\\varphi(1+x\,.\,1+y\,.\,1+z)-\varphi(1+w\,.\,1+y\,.\,1+z)-\varphi(1+w\,.\\1+x\,.\,1+z)-\\\varphi(1+w\,.\,1+x\,.\,1+y)+\varphi(1+w\,.\,1+x\,.\,1+y\,.\,1+z).\end{aligned}$$

Assume $\varphi(1+x)=A+A'x+A''x^2+\ldots\ldots+A''^{\ldots n}x^n+$ and make the requisite substitutions; we shall find $A=o$, A', A'' and A''' arbitrary. Then to have the law of the coefficients, find, according to the rule, the coefficient of x in the coefficient of z in the coefficient of y, and, comparing in this the coefficients of the powers of w, we find

$$n(n-1)(n-2)A''^{\ldots n}+3(n-1)(n-2)^2.A''^{\ldots(n-1)}+(3.\overline{n-2}^2.\overline{n-3}+\overline{n-2})A''^{\ldots(n-2)}+(\overline{n-2}.\overline{n-3}.\overline{n-4}+n-3)A''^{\ldots(n-3)}=0$$

If we take successively 4, 5, 6, &c. for n we find

$$\begin{aligned}\varphi(1+x)=&A'\left\{x-\tfrac{1}{24}x^4+\tfrac{9}{120}x^5-\right\}\\&+A''\left\{x^2-\tfrac{7}{12}x^4+\tfrac{11}{12}x^5-\right\}\\&+A'''\left\{x^3-\tfrac{3}{2}x^4+\tfrac{7}{4}x^5-\right\}\end{aligned}$$

The series at bottom is $L^3(1+x)$; the series next above it is $L^2(1+x)+L^3(1+x)$. To find the upper series we have the same means as in the last Problem, $L(1+x)$ being a particular value of $\varphi(1+x)$; make then $A'=1$, $A''=-\frac{1}{2}$, $A'''=\frac{1}{3}$; our equation becomes

$$\begin{aligned}L(1+x)=&x-\tfrac{1}{24}x^4+\tfrac{9}{120}x^5-\\&-\tfrac{1}{2}\left\{L^2(1+x)+L^3(1+x)\right\}\\&+\tfrac{1}{3}L^3(1+x)\end{aligned}$$

whence $x-\frac{1}{24}x^4+\frac{9}{120}x^5-$ &c. $=L(1+x)+\frac{1}{2}L^2(1+x)+\frac{1}{6}L^3(1+x)$, and

$$\begin{aligned}\varphi(1+x)=&A'\left\{L(1+x)+\tfrac{1}{2}L^2(1+x)+\tfrac{1}{6}L^3(1+x)\right\}\\&+A''\left\{L^2(1+x)+L^3(1+x)\right\}\\&+A'''.\ L^3(1+x)\end{aligned}$$

which is the complete solution of the proposed equation.

As, however, this result has been obtained by the inspection of only a few terms of two series, a doubt may be entertained with respect to its truth: make therefore $w=x=y=z$, in equation (3), it will become $4\varphi(1+x)=6\varphi(1+x)^2-4\varphi(1+x)^3+\varphi(1+x)^4$, which by the substitution of the

value found above for $\varphi(1+x)$ is found to be identical, and the truth of the solution is proved.

We may now attempt the solution of the general problem, viz. Let $x, x', x'', x''', \ldots x'^{\cdots p}$ be independent quantities; it is required to find $\varphi(1+x)$ from the following equation,

$$\Sigma.\varphi(1+x''^{\cdots m})=\Sigma.\varphi(1+x''^{\cdots m}.\,1+x''^{\cdots n})-\Sigma.\varphi(1+x''^{\cdots m}.\,1+x''^{\cdots n}.\,1+x''^{\cdots r})+$$
$$\Sigma.\varphi(1+x''^{\cdots m}.\,1+x''^{\cdots n}.\,1+x''^{\cdots r}.\,1+x''^{\cdots s})-\Sigma.\varphi(1+x''^{\cdots m}.\,1+x''^{\cdots n}.\,1+x''^{\cdots r}.\,1+x''^{\cdots s}.\,1+x''^{\cdots t})+\&c.$$

Assume as usual $\varphi(1+x)=A+A'x+A''x^2+A'''x^3+$; but instead of attempting to find the law of the coefficients, we may easily convince ourselves that $\varphi(1+x)$ will have the following form, viz.

$$\begin{aligned}\varphi(1+x)=A'&\left\{L(1+x)+\alpha''L^2(1+x)+\alpha'''L^3(1+x)+\alpha''''L^4(1+x)+\ldots\ldots+\alpha''^{\cdots p}L^p(1+x)\right\}\\
+A''&\left\{L^2(1+x)+\beta'''L^3(1+x)+\beta''''L^4(1+x)+\ldots+\ldots\ldots\beta''^{\cdots p}L^p(1+x)\right\}\quad(4)\\
+A'''&\left\{L^3(1+x)+\gamma''''L^4(1+x)+\ldots\ldots+\gamma''^{\cdots p}L^p(1+x)\right\}\\
&\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\\
+A'^{\cdots p}&L^p(1+x)\end{aligned}$$

This form evidently includes, as particular solutions,

$$L(1+x),\ L^2(1+x),\ L^3(1+x),\ \ldots\ldots\ L^p(1+x):$$

and, by means of these particular solutions, we are enabled to find the coefficients α'', α''', α'''', &c. β''', β'''', &c., γ'''', &c. &c. For let

$$\begin{aligned}L(1+x)&=x+b''x^2+b'''x^3+b''''x^4+\\
L^2(1+x)&=\qquad\quad x^2+c'''x^3+c''''x^4+\\
L^3(1+x)&=\qquad\qquad\qquad x^3+d''''x^4+\\
&\&c.\qquad\qquad\qquad\&c.\end{aligned}$$

By changing A′, A″, A‴, &c. into the coefficients of each of these expressions, successively, we have p particular values of equation (4); viz.

$$L(1+x)=L(1+x)+\alpha''L^2(1+x)+\alpha'''L^3(1+x)+\alpha''''L^4(1+x)+ \ldots\ldots+\alpha''^{\cdots p}L^p(1+x)$$
$$+b''\left\{L^2(1+x)+\beta'''L^3(1+x)+\beta''''L^4(1+x)+\ldots\ldots+\beta''^{\cdots p}L^p(1+x)\right\}$$
$$+b'''\left\{L^3(1+x)+\gamma''''L^4(1+x)+\ldots\ldots+\gamma''^{\cdots p}L^p(1+x)\right\}$$
$$+b''''\left\{L^4(1+x)+\ldots\ldots+\delta''^{\cdots p}L^p(1+x)\right\}$$
$$\cdots\cdots\cdots\cdots\cdots\cdots$$
$$+b''^{\cdots p}\times L^p(1+x)$$

from which we derive the equations $b''+\alpha''=o$, $b'''+b''\beta'''+\alpha'''=o$, $b''''+b'''\gamma''''+b''\beta''''+\alpha''''=o$, &c.; next we have

$$L^2(1+x)=L^2(1+x)+\beta'''L^3(1+x)+\beta''''L^4(1+x)+\ldots\ldots+\beta''^{\cdots p}L^p(1+x)$$
$$=c'''\left\{L^3(1+x)+\gamma''''L^4(1+x)+\ldots\ldots+\gamma''^{\cdots p}L^p(1+x)\right\}$$
$$+c''''\left\{L^4(1+x)+\ldots\ldots+\delta''^{\cdots p}L^p(1+x)\right\}$$
$$\cdots\cdots\cdots\cdots\cdots\cdots$$
$$+c''^{\cdots p}\times L^p(1+x)$$

whence we get the equations $c'''+\beta'''=o$, $c''''+c'''\gamma''''+\beta''''=o$, &c.

The next particular solution is

$$L^3(1+x)=L^3(1+x)+\gamma''''L^4(1+x)+\ldots\ldots+\gamma''^{\cdots p}L^p(1+x)$$
$$+d''''\left\{L^4(1+x)+\ldots\ldots+\delta''^{\cdots p}L^p(1+x)\right\}$$
$$\cdots\cdots\cdots\cdots\cdots\cdots$$
$$+d''^{\cdots p}\times L^p(1+x)$$

whence $d''''+\gamma''''=o$, &c. and by proceeding in the same way we have as many equations as the coefficients which are to be determined.

So much for the expansion of these functions.

What Mr. SPENCE means by the note in page ix. of his Preface, where he speaks of the integral $\int\!\!\int\frac{d^n x}{x} = \varphi\,(x)$ an equation which is evidently impossible, I am unable to form the smallest conjecture.

Papcastle, March 3, 1817.

XVIII. *On the passage of the ovum from the ovarium to the uterus in women.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read May 1, 1817.

No subject connected with physiology has more employed the attention of the anatomist and philosopher than the first formation of the embryo in the class mammalia, and yet even at this day, when the same subject has been completely investigated in oviparous animals, and it is known that an ovum is formed in the ovarium of the quadruped, the circumstances respecting its impregnation have not been ascertained.

The great Harvey, although supplied by the munificence of his king with deer in all the different stages after being fit for the male, was unsuccessful. John Hunter, who prosecuted the same enquiry in the ewe, also failed. His brother, Dr. William Hunter, in his splendid work on the Gravid Uterus, has given the most correct representations of the human embryo from the end of the third week till the time of birth, but has not said any thing upon the subject of impregnation.

Haighton and Cruikshank, by experiments on rabbits, confirmed the opinion of De Graaf,* that an ovum is carried from the ovarium into the uterus, but by mistaking the corpus

* De Graaf's observations are mentioned in the 7th Volume of the Phil. Trans. p. 4052. In the same volume, p. 4018, Dr. Kerkringius's observations concerning eggs to be found in all sorts of females are noticed.

luteum for the effect of impregnation, instead of the substance in which the ovum is formed, which at that time was the generally received opinion, got entangled in theoretical opinions, which misled them in their farther enquiries.

In this state of our knowledge upon this most interesting subject, accident has done what no predetermined experiments had accomplished, it has enabled me to detect the ovum in the human uterus. It is so small, that had not the uterus been previously hardened in spirit as well as the ovum itself, it probably would have escaped observation, and, after it was found, it could not have been identified to be the ovum from which a child was to be produced, had it not been brought under the eye of Mr. Bauer, the only person, I may say, in this or any other country, who could so correctly apply to it the powers of the microscope, as to determine its form; could so separate its parts on the field of the microscope, as to display its organization; and so delineate what he saw, as to convey distinct notions that it was the first rudiments of a child.

I shall first give the history of the woman's case from the time of her impregnation, and then detail the appearances that were met with in the uterus and ovaria, after death.

A servant maid, 21 years of age, had been missing from her master's house, on the 7th of January 1817, for several hours in the forenoon; she came home in high spirits, said she had bought a pair of corsets and some other parts of dress.

In the evening, she got her fellow servant to assist her in putting on the corsets, but on lacing them she complained of being sick, and all over unwell; on taking some brandy she

recovered a little, and went to bed. Next day she was much the same; the period of menstruation had arrived, but it did not come on, and from this time there was a wildness in her manner, and she appeared distressed in her mind. On the 13th she had an epileptic fit attended with delirium, and on the 15th, about ten o'clock in the forenoon, died.

After death, the uterus showed signs of pregnancy, and from the statement that has been given, she appears to have been impregnated on the 7th of January, eight days before her death; for, although she was known to have a lover, there are circumstances to prove, that she could not have seen him after that time, nor for many days before.

The uterus having been hardened in spirit, with the assistance of Mr. Clift, I examined the parts. The right ovarium had a small torn orifice upon the most prominent part of its external surface, we slit it open in a longitudinal direction, in a line close to the edge of this orifice; the orifice was found to lead to a cavity filled up with coagulated blood, and surrounded by a yellowish organized structure. Upon opening into the cavity of the uterus, its inner surface was covered with an exudation of coagulable lymph, beautifully represented in the drawing; (Pl. VIII.), the ovum lay concealed among the long fibres of coagulable lymph near the cervix, and was brought to view by separating them with the point of a needle which I employed in making the search. As soon as it was disentangled, it rose up, moving along with the loose ends of the fibres into the spirit, by which the parts were covered. It had an oval appearance, one portion of it was quite white, the other semi-transparent; but soon after, being exposed to the spirit, the whole

became opaque. The os tincæ was entirely shut up with a strong solid jelly, the two orifices at the angles of the uterus, by which it communicates with the Fallopian tubes, were both pervious.

As the ovum was so extremely small as to admit of dispute, whether it was one or not, I carried it immediately to Kew to Mr. BAUER, who, after examining it, said that it looked like the egg of an insect. His drawings of the ovum and uterus, show to what an excellence microscopical observations can be carried, since in so small a particle of animal matter, he has pointed out the effects of impregnation even before any part of the vascular system had been formed, and where only the two projecting points within the ovum had been marked out as the future situations of the heart and brain. These two points are still to be distinguished in the ovum in a dried state, and that towards the broadest end is the largest.

Small as this ovum is, it bears a very fair proportion to that represented by Dr. HUNTER at the end of three weeks; and, had this woman lived twenty-four hours longer, the ovum would probably have in that time been united to the fibrous structure surrounding it, and appeared secluded from the cavity of the uterus in the same degree as Dr. HUNTER's is represented to be.

The corpus luteum has always been considered as the effect of impregnation, and a certain mark of conception having taking place, as I have already observed; but in this case there was not only the corpus luteum belonging to the present conception, but one still more distinct in the middle of the ovarium. This unexpected appearance of two corpora lutea, made me enquire farther into the subject, and led me to discover that the corpus luteum, in its origin, is a solid

compact glandular substance in which the ovum is formed, and, after the ovum is expelled, the blood which fills up the cavity is gradually absorbed, leaving a small cavity, which marks the place where the ovum had been.

Upon examining the ovaria of several women who had died virgins, and in whom the hymen was too perfect to admit of the possibility of impregnation, there were not only distinct corpora lutea, but also, as will be found in the present case, small cavities round the edge of the ovarium, evidently left by ova that had passed out at some former period, so that this happens during the state of virginity; and, as in Mr. CRUIKSHANK's experiments, the fimbriæ of the Fallopian tube of the rabbit in heat, were found embracing the ovarium, although she had not received the male, we cannot doubt, that every time a female quadruped is in heat, one or more ova pass from the ovarium to the uterus, whether she receives the male or not.

These facts explain the error which physiologists have gone into, of mistaking the corpus luteum, in which another ovum is forming, for that which belonged to the ovum of the present conception, and which at the time of delivery has disappeared.

Mr. BAUER's drawings not only show the changes which take place in the ovarium, for the purpose of forming the ova, but also the internal surface of the Fallopian tube at the time the ovum passes along it in its course to the uterus, which I believe has never before been represented.

The appearances are so clearly shown in the drawings, that it is not necessary to describe them: I shall therefore confine myself to an explanation of their probable uses.

The dilatation of this tube, at a small distance from the

fimbriæ, appears to be fitted for the reception of the ovum as well as of the semen, and the ovum is probably retained in this situation for several days, to prolong the opportunity of its being impregnated.

It has been disputed, whether the semen ever comes in contact with the ovum, or even arrives at the uterus; but as Mr. HUNTER has proved by experiment that it reaches the uterus,* and as there is no impediment to its passage from that organ to the ovarium, it must be admitted that the semen reaches the ovum before impregnation can take place.

The formation of ova in the ovaria, and their appearing in that organ in succession, joined to the circumstance of animals during the warm season being ready to receive the male once a month, leads to an opinion very contrary to that which is commonly received respecting menstruation. This discharge has been supposed a previous step, preparing the uterus for uterogestation; and if a woman has not been impregnated soon after menstruation, it is presumed that she may be more fortunate after her next period.

It is clear from the case which has been stated, that such periods are totally unconnected with the formation of the ovum, the process of its leaving the ovarium, or its impregnation; but, if impregnation does not take place, such a discharge may be necessary for the relief of parts to which there had been so great a derivation of blood, as the only means of restoring them to their natural state. The uterus in women and in the monkey has a more compact form than in other animals, which may explain the circumstance of menstruation being confined to them.

* J. HUNTERUS canis fœminæ inter coeundum occisæ, uterum aperuit; quo facto maris semen in ipsum uterum, per saltus intromissum, clare vidit.

In proof of menstruation not being necessary for impregnation, I shall mention the following case. A young woman was married before she was seventeen, and, although she had never menstruated, became pregnant; four months after her delivery she became pregnant a second time, and four months after the second delivery she was a third time pregnant, but miscarried; after this she menstruated for the first time, and continued to do so for several periods, and again became pregnant.

I have given Mr. BAUER's account of the ovum, and the drawings he has made of it, in his own words, than which none can be more clear or satisfactory.

"On closely examining the subject under the microscope, I found it consisted of membrane, which, considering the extreme minuteness of the subject, is of considerable thickness and consistence, very little transparent, quite smooth, and milk white, forming a kind of bag or pouch of an irregular oval shape, not quite $\frac{12}{200}$ parts of an inch in length, and in its middle about $\frac{9}{200}$ parts of an inch broad; on one side it has an elevated ridge or large fold along the whole length, and on the opposite side it is open nearly the whole length, but has no appearance of being torn, the edges of the membrane being smoothly rolled inwards, which gives it much the shape of a little shell of the genus Voluta.

"When laid on glass, the membrane admitted easily to be laid open on both sides, with the point of a fine camel hair pencil. When thus opened, I found it contained another smaller bag somewhat less than $\frac{18}{200}$ parts of an inch long, and not quite $\frac{5}{200}$ parts of an inch broad, ending at the upper extremity nearly in a point, but the under extremity was very obtuse or truncate, and in the middle it was slightly

contracted, which gave it the appearance of a young seed capsule of some plants that contain only two seed kernels.

"This inner bag consisted of a seemingly very thin, perfectly smooth, and glossy membrane, which seemed to have considerable strength, as it bore to be rubbed pretty strongly, not only with the camel hair pencil, but also with the point of the quill; it seemed to be filled with some thick slimy substance, as an impression made on it with the point of the quill remained for a considerable time visible: it contained two round corpuscules, apparently more opaque, and of a yellowish tint; they were not only visible through the transparent membrane, but they swelled the membrane over them, so that the light and shade made them to be distinctly seen; and by slightly pressing the bag with the quill between the two corpuscules, they could be separated to a greater distance from each other, but on putting more moisture upon the subject, they returned quickly to their former position. This little bag was along its whole length, with its back part strongly fixed to the outer membrane, at least I could not remove it with the camel hair pencil, and more force I was afraid to employ.

"I attempted to open the little bag, if possible, to extract the corpuscules, but on piercing, with the point of a very small needle, the upper extremity, a thick slimy matter, like honey, came out, and with the membrane adhered to the needle, so that I could no farther proceed; and fearful of spoiling the whole; I gave up the attempt, and left the subject on the glass to dry; but I observed, as the spirit and moisture gradually evaporated, so the little bag flattened, and, as if melting, shrunk into the outer membrane, and almost disap-

peared, but in a strong light was still visible in the microscope.

"When quite dry, its colour changed to a light yellowish brown, and it lay quite loose on the glass, except at the upper extremity, where I attempted to open it; it was strongly glued to the glass, and it required several times to be moistened at that part with water, to remove it from the glass.

"I have now placed it between two pieces of talc in an ivory slider; and in a strong light the two corpuscules may still be seen through a common magnifying glass."

The drawing of the uterus (Pl. VIII.) is of the natural size: the parts are so distinct that no letters of reference appear to be necessary to point them out. The ovum is shown exactly in the spot in which it was discovered, with the appearance which it at that time put on.

The drawings of the ovaria and Fallopian tubes are magnified four times, to give a more exact notion, than could be otherwise done, of the canal through which the ovum passes, before it arrives at the cavity of the uterus. The appearance the corpora lutea put on, is the most exact representation from nature. In the right ovarium, cells remain where former ova had been formed, and one corpus luteum, which is cut through the middle, has made considerable advance in its formation, another appears to be in a much earlier stage, all the different orifices are the transverse and oblique sections of blood vessels.

In the left ovarium, the opening through which the ovum, the subject of the present Paper, passed out, is distinctly seen, and the cavity in which it was contained, is filled with coagulated blood in a laminated form; behind this, the glandular

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Fig. 1.

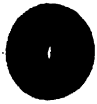


Fig. 2.

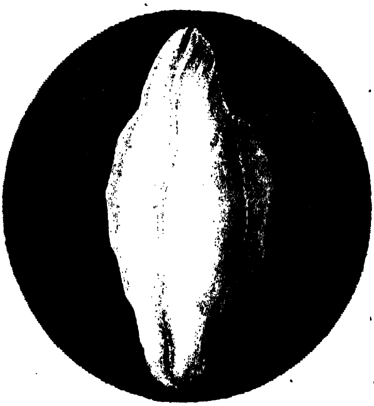


Fig. 3.

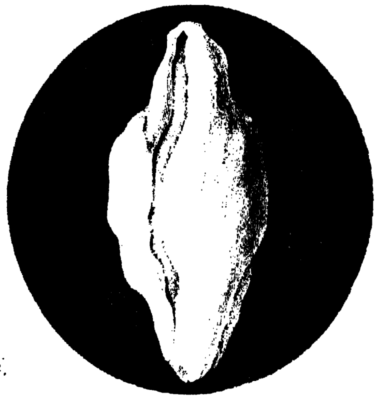
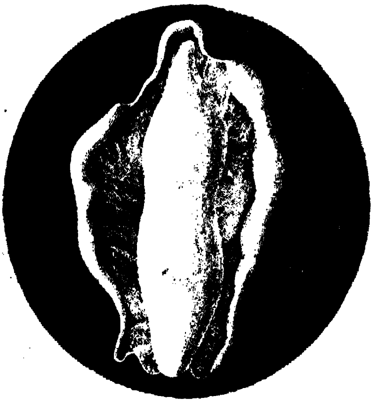


Fig. 4.



Franz Bauer. del.

J. Basire sc.

structure of the corpus luteum is readily distinguished, of the form of an irregular oval. Towards the other end of this ovarium is the transverse section of a corpus luteum, not far enough advanced in its growth to have a cavity in which the future ovum is to be generated, the whole having one uniform glandular structure, the general form, like that of the other, being irregularly oval. The orange colour, peculiar to the glandular structure of the corpora lutea, which is very bright, and forms their distinguishing character, is not given, as the drawing is intended to be engraved.

Explanation of Plates VIII, IX, X, XI.

Plate VIII.

The uterus laid open from behind, showing the ovum in the situation in which it was discovered. The other parts require no description.

Plate IX.

The right ovarium laid open, showing the orifice at which the ovum escaped. The Fallopian tube laid open through its whole length. All these parts are minutely described in the Paper.

Plate X.

The left ovarium and Fallopian tube laid open.

Plate XI.

Different views of the ovum.

Fig. 1. The ovum represented of its natural size.

Fig. 2. The ovum magnified, exposing that side which is covered by one uniform membrane.

Fig. 3. The opposite side exposed, to show that the external membrane is disunited through its whole length, and unattached to the one under it.

Fig. 4. The two disunited edges of the outer membrane turned aside, exposing the inner membrane, through which are seen two very slight protuberances, the probable seat of the future heart and brain.

XIX. *Some farther observations on the use of the Colchicum Autumnale in Gout.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read May 8, 1817.

I LAID before the Society, some experiments and observations in favour of this medicine acting upon the gout through the mediu of the circulation, and not by its effects directly upon the stomach and intestinal canal.

The object of the present Paper is to show that the infusion throws down a deposit, the separation of which does not appear to diminish the specific effects upon the gout, and renders those upon the stomach and intestines milder than when the deposit is taken along with the infusion.

The bulb of the Colchicum Autumnale contains a certain quantity of extractive matter, and a large portion of mucilage, both of which are taken up by the wine, in the first instance; when the strained liquor is allowed to stand, a considerable deposit almost immediately takes place.

In the first trials that were made with this medicine in St. George's Hospital, it was natural to enquire whether this deposit contained any medical virtues, and upon trials frequently repeated, it was found to have none.

This led to the opinion that the extractive matter suspended in the wine, was alone the active part of the medicine, and not only the first deposit was inert, but also that which from time to t me was afterwards found to take place.

Of this opinion I was led to entertain considerable doubts, in consequence of having found upon one occasion, in which I took half a bottle of the Eau Medicinale which had been poured off without shaking the bottle, that the sensible effects were very mild; those produced by the other half, in which the deposit was mixed, were unusually severe, the nausea being greater, and a greater number of stools being produced.

These doubts were much strengthened, when I found that the effects of the Eau Medicinale are more violent upon many stomachs than those of the vinous infusion of the Colchicum, which probably arises from the Eau Medicinale being kept in small bottles, in consequence of which all the deposit that takes place is given along with the infusion, while the vinous infusion of Colchicum being kept in large bottles, the deposit falls to the bottom. If such deposit increased the powers of the medicine in counteracting the symptoms of gout, it would be unnecessary to prosecute this investigation farther, since it would be absurd to diminish the violence of a medicine, if, by so doing, its efficacy is to be diminished in an equal degree.

To ascertain this point, I gave 60 drops of the vinous infusion of Colchicum, in which there was no deposit whatever, to a man labouring under a severe paroxysm of gout, to which he was a great martyr, and whose paroxysms were usually of several weeks continuance; he was 60 years of age.

The medicine was exhibited on the 17th of January 1817, his pulse being 115. In half an hour, he had slight nausea, which soon went off. In 5 hours, a profuse perspiration came on, and the pain of the gout entirely subsided, leaving a soreness in the parts that had been affected. In 12 hours, the bowels were gently moved, his pulse 105 and irregular;

in 14 hours, his bowels were acted on a second time; in 19 hours, his pulse was 92, and natural; in 48 hours, he was quite well, and has continued so, a period of more than three months.

The result of this case satisfied me, that the infusion contained the specific remedy for the gout, and that the deposit is not necessary for its removal.

This rendered it probable that, where the deposit is taken along with the infusion, its solid form prevents it from being carried into the circulation of the blood, and it remains in the stomach, producing more or less mischief in that viscus, without being any way concerned in driving away the disease for which the medicine was exhibited; in this respect resembling many of the salts of mercury, which irritate the bowels, without relieving the symptoms of the venereal disease.

I explained these opinions to Mr. GATCOMBE, who gives me his assistance in my professional pursuits, and requested him to investigate this subject.

To do this more completely, he began by repeating the three experiments detailed in my former Paper, substituting the Eau Medicinale for the vinous infusion of Colchicum, so as to determine with more precision, whether they are or are not the same medicine.

Exp. 1. Thirty drops of the Eau Medicinale with the deposit, were injected into the jugular vein of a dog; the effects were the same, as in my experiment with the same quantity of the vinous infusion of Colchicum, only the animal was 2 hours longer in recovering from them, and was purged for 9 hours afterwards.

Exp. 2. Sixty drops of the Eau Medicinale, were given by

the mouth to the same dog: the effect was less, than in my experiment with the vinous infusion of Colchicum exhibited in the same quantity: this arose from a very copious evacuation of urine having been produced.

Exp. 3. One hundred and sixty drops of the Eau Medicinale, injected into the jugular vein of a dog, produced rather more violent effects, than in my experiment with the same quantity of vinous infusion of Colchicum; the animal died in 6 hours, and after death the appearances of inflammation in the bowels, were more violent, approaching to mortification.

Mr. Gatcombe having found so exact a similarity in the effects of the two medicines, in these trials, I requested him to make the following comparative experiment on the effects produced upon the stomach and bowels by the Eau Medicinale, in which there is a deposit, and the vinous infusion of Colchicum, in which there is none.

Exp. 4. One hundred and sixty drops of the Eau Medicinale, taken by the mouth, produced the same effects, and left the same appearances after death, as when that quantity was injected into the vein, only the animal lived 9 instead of 6 hours.

One hundred and sixty drops of the vinous infusion of Colchicum, were given to a puppy of the same litter; they produced vomiting, purging, and a great flow of urine; but the animal very soon recovered.

Two hundred drops of the same infusion, after an interval of several days, were given to the same dog, and the effects were the same; the dog had become much improved in his looks and condition.

Three hundred drops, after an interval of several days, were given to the same dog: effects, corresponding with

those of 160 drops of the Eau Medicinale, were produced. The dog died in 9 hours, and the appearances of inflammation after death were of the same kind, but not nearly so extensive.

From these experiments the Eau Medicinale with the deposit, produces double the irritation on the coats of the stomach and intestines, that is brought on by the vinous infusion of Colchicum: this probably arises from the local inflammation brought on by the deposit, upon the internal membrane of these viscera.

To determine as nearly as possible the effects of the deposit, when applied in a solid form, to the coats of the stomach and intestines, the following experiment was made.

Exp. 5. Six grains of the deposit of the vinous infusion of Colchicum, were given to a dog in bread and milk; in 3 hours it produced vomiting and purging, which lasted 24 hours; during the latter part of that time, there was blood in the stools, as well as in what was brought up from the stomach.

I wished to repeat this experiment with the deposit from the Eau Medicinale, but found in bottles that had been kept 7 years, the wine had become vapid, and, in this decomposed state, the acrid part of the deposit had been taken up again; so that in 12 bottles, containing different quantities, only 5 grains could be procured, which was quite inert.

Being at a loss to know whether the extractive matter deposited from the infusion, is in reality more acrid to the stomach than that suspended in it, or the circumstance of its being applied in a solid form renders it so, I requested Professor BRANDE to acquaint me, if it could be the effect of any chemical decomposition having taken place.

He favoured me with the following explanation, which is highly satisfactory. " There are certain vegetable bodies which, when infused in water, or diluted spirit, furnish a solution which lets fall a sediment, in which their activity, as purgative medicines, chiefly resides; this is remarkably the case with the wild cucumber or elaterium. The sediment is a very drastic purge; the part that remains dissolved is comparatively mild in its operation upon the bowels." This explanation of Professor BRANDE applies to the Colchicum, and we are now enabled to separate the purgative qualities of the vinous infusion of Colchicum and Eau Medicinale, from those which prove a specific for the gout, in the simplest possible manner, by keeping them in large bottles, instead of small ones, and not going too near the bottom.

It also explains what is asserted by PROSPER ALPINUS,* that the Egyptian women eat the fresh bulbs, that they may grow fat; an effect which was found to take place in the dog, while the dose was confined within such limits as not to act too violently upon the bowels.

The bulbs of the Egyptian Colchicum, when long kept, weigh one dram each; on being steeped in water they double their weight; so that the quantity of extractive matter contained in two or three recent bulbs, while combined with the mucilaginous matter, of which the bulbs are principally composed, is not likely to be sufficient to do more, than act as a brisk purgative, the occasional use of which tends to make people grow fat.

Since this Paper was read, the patient who is mentioned as having had the gout in January, has had another attack:

* Hist. Nat. Egypt. pars. 1. lib. 3. cap. 14.

it came on the 10th of July, and was removed in the same manner as the former, by the same dose of the medicine. The President of the Society also, convinced by the evidence contained in this and the former Paper, that the Vinum Colchici, in which there is no deposit, must be a less hurtful medicine than the Eau Medicinale, thought it a duty to himself and the public to make trial of it, and on the 20th of July, when the gout in his left hand and the whole of the joints of that side of the body was very severe, allowed me to give him 90 drops of the Vinum Colchici, and found that the symptoms of gout were sooner and more completely removed than they ever had been by the Eau Medicinale, of which he has an experience of seven years, having taken it regularly ever since the 17th of February, 1810, and during that time kept a regular account of the doses, their effects, and the intervals between them.

XX. *Upon the extent of the expansion and contraction of timber in different directions relative to the position of the medulla of the tree. By* Thomas Andrew Knight, *Esq. F. R. S. In a Letter addressed to the Right Hon. Sir* Joseph Banks, *Bart. G. C. B. P. R. S.*

Read May 8, 1817.

My Dear Sir,

Many attempts have been made by writers on vegetable physiology, to account for the force with which the sap of trees has been proved by Hale to ascend during the spring, without any hypothesis having been offered, which has been thought satisfactory: and almost all, which have been offered, have been justly rejected as wholly inadequate. I have suggested in the Philosophical Transactions of 1801, 2d Part, page 333, the expansion and contraction of those cellular processes, which proceed from the bark to the medulla, which I have there called the true, or silver grain of the wood; and which have, generally, though most improperly, been called medullary processes. I have there shown, that this substance expands and contracts very considerably under changes of temperature and moisture; and I have stated that a board of oak, which has been formed by cutting across the supposed medullary processes, can scarcely be made, by any means, to retain the same form and position when subjected to various degrees of heat and moisture. I had not at that time ascertained, with accuracy, the comparative expansion

and contraction of timber, when divided in different directions relative to the medulla of the tree, and I was not in possession of any fact which enabled me to prove the existence of any such power, in a state of action, in the living tree. But experiments, which I have made at different subsequent periods, have afforded very satisfactory evidence of the presence of this power in a state of action in living trees, and have also enabled me to ascertain some facts, which appear interesting, and likely to prove useful in directing the proper mode of application of wood for various purposes, in which it is important that it should permanently retain its primary extent and form. These experiments were made upon timber of many different kinds; but as the results were all very nearly the same, I shall confine myself to those made upon the oak, the ash, the beech, and poplar.

Some thin boards of the wood of two of the above mentioned species of trees, the ash and the beech, were cut in opposite directions relative to their medulla, so that the convergent cellular processes crossed the centre of the surfaces of some of them at right angles, and lay parallel with the surfaces of others; by which means I became enabled to mark the comparative extent of their expansion and contraction when they were subjected to various degrees of heat and moisture. Both were placed under perfectly similar circumstances in a warm room, where those, which had been formed by cutting across the convergent cellular processes, soon changed their form very considerably, the one side becoming hollow, and the other raised; and in drying these contracted nearly fourteen per cent. relative to their breadth. The others retained, with very little variation, their primary form, and did not contract more than

three and a half per cent. in drying. Both were, subsequently, several times subjected to various degrees of temperature and moisture, and each expanded nearly in the same degree that it had contracted, the form of the one remaining very nearly permanent, and that of the other constantly changing.

A beech and an ash tree, each somewhat exceeding twenty inches in diameter, were felled in the end of January (at which time the buds of both had become sensibly enlarged) and a transverse section of about an inch in thickness, and necessarily of a circular form, was immediately cut off from the trunk of each, near its base. An incision was then attempted to be made with a saw from the bark to the medulla, directly in the line of the convergent cellular processes, with the expectation that these, on each side, would expand, and impede the action of the saw. The result was just what I had anticipated, and long before the saw approached near the medulla, it became so strongly compressed that my assistant could scarcely move it. A much thinner saw, which I had in readiness, was then employed; and the incision, which was kept open by a wedge, was extended to the medulla. The wedge was then withdrawn, and the opposite sides of the division instantly came in contact with great force. A second incision, similar to the preceding, was then made to commence at the bark, about an inch distant from the preceding, and to terminate, like that, at the medulla; by which means, a wedge of wood, an inch square at the bark, and ending in an edge at the medulla, and ten inches in length, was wholly detached. This, nevertheless, did not quit its position, being retained in it by the expansion of the wood from which it had been separated.

The opposite sides of the same transverse sections of wood were divided by the saw in a direction diametrically opposite to that above mentioned; under which circumstances, the expansion of the convergent cellular processes could not, as in the preceding cases, occasion any pressure upon the sides of the saw, which consequently continued to move with perfect freedom.

These circumstances led me to infer, that the medullary canal must be subject to considerable variations of diameter, with the increase or diminution of the quantity of moisture in the wood; and I conceived, that I should easily be able to ascertain the truth or falsehood of this conjecture by the following means. I selected, in winter, some parts of the stems of young trees as soon as they were felled, which I retained in such a situation as might occasion them to lose a considerable part of the water they contained, though not to such an extent as to destroy, or endanger, life. The medulla of these was then removed; and the space it had occupied was filled with cylindrical pieces of metal, which were so large that they could not be introduced without considerable force. The pieces of wood were then deposited in a damp soil, from which they absorbed much moisture; and at the distance of ten days, I found the medullary canal so much enlarged, that the pieces of metal dropped through without any pressure being applied.

I am prepared to prove, in a future communication, that the quantity of moisture in the alburnum is subject to great variations in the living tree, and therefore I conclude, that the medullary canal frequently changes the extent of its diameter.

It appears probable that, by means of this kind of expansion, the internal parts of timber trees so frequently become rifted or cleft. Winds have been assumed by some, and frost by others, as the cause of these injuries. But winds cannot possibly be the cause, as pollared oak trees, upon which these can exert but very little power, are almost always rifted; and the frost of this climate is rarely, or never, sufficiently intense to congeal the winter sap of trees. This agent must also, I conceive, act suddenly, if it act at all, and the trunks of large oaks can not suddenly be cleft asunder in silence. The oak timber of England is also much more frequently rifted than that of the north of Europe. The force with which the cellular substance of timber expands, is fully equal to produce the preceding effects. I have often seen it overcome the pressure of many tons: it is therefore greatly more than equal to give the impulse to the sap, which was observed by HALE; and as it is obviously in action in the living tree, I must retain the opinion which I formerly gave, that it is the agent by which motion is given to the ascending fluid. How it immediately acts upon the passages through which that fluid ascends, and whether that fluid passes through the cells themselves, or through the intercellular passages described in the elaborate work of Dr. KIESER,* I confess myself to be wholly ignorant, and the slow motion of the fluid, the excessive minuteness of the passages, and the varieties of directions in which it is often moving at one and the same time, will ever render this a question of extremely difficult solution.

There is another kind of contraction in timber whilst drying, and of expansion when subsequently wetted or moistened,

* Mémoire sur l'Organization des Plantes.

which is observable only in lifeless wood; and which has apparently no connection with the power by which the sap is raised in the living tree. The interior and older layers of wood are much more solid and specifically heavy, than the external layers in the same tree; and the latter, consequently, contract more longitudinally in drying than the former, and the edge of every board (that has been cut with surfaces nearly parallel with the line of the convergent cellular processes) which lay nearest the medulla in the tree, will therefore in drying become convex, whilst the opposite edge will become concave. The ill effects of this are often felt when oak timber is employed to form joists, part of these in drying always rising above, and others sinking below the first and proper position. The cause of some musical and other instruments being put out of order by changes of weather, whilst others, apparently similarly constructed, are free from such defects, may probably be traced to one of the sources abovementioned.

I am, my dear Sir, &c.

T. A. KNIGHT.

Downton, April 26, 1817.

The Right Hon. Sir Joseph Banks, Bart. G. C. B. P. R. S.

XXI. *Observations on the temperature of the ocean and atmosphere, and on the density of sea-water, made during a voyage to Ceylon. In a Letter to Sir* Humphry Davy, *LL. D. F. R. S. By* John Davy, *M. D. F. R. S.*

Read May 22, 1817.

My Dear Brother,

According to the promise contained in a former letter, I proceed to give you a short account of the observations which I made during my late voyage from England to Ceylon. At present, I shall confine myself chiefly to three topics, the specific gravity of the water of the ocean, and its temperature, and the temperature of the atmosphere; subjects of some importance in the natural history of our globe; and in which, I know, you are interested. Incidentally I shall notice the height of the barometer, the direction of the winds, and the state of the weather.

For the sake of brevity, I shall present the principal results of my observations in the form of a table, to which I shall add some explanatory notes and general remarks.

Time.	Latitude by Observation.	Longitude by Chronometer.	Specific gravity of sea water at temp. 80.	Maximum temp. of the air in the 24 hours.	Minimum.	Mean.	Maximum temp. of the sea in the 24 hours.	Minimum.	Mean.	Barometer.	Winds.	Weather, &c.
	° ′	° ′		° ′	° ′	° ′	° ″	° ′	° ′	° ″		
Feb. 12	N. 9. 1	W. 6,30				40,			48,		South	Clear.
14	48,28	10,20	10251	51,	49,	50,			51,	29,	SW	Cloudy with occasional sunshine.
15	47,53	12,28	10264	52,	50,	50, 7			52,	30,1	WNW	
16	46,28	13,50	10256	59, 5	49,	50,			52,	30,	North	
17	44,32	14,10		50,	49,	49, 6			52,	30,	NE	
18	42,54	15,47	10256	53,	51,	52,			53,	30,	ENE	
19	40,48	16,34	10256	53,	52,	52,25			53,	30,	SE	
20	40,21			56,	53,	54, 3			55,			Clear. A complete calm.
21	40,12			58,	56,	56, 6			56,		SSW	Cloudy, wind strong, sea rough.
24	39,24			57,	55,	56,			58,	30,4	NE	Clear, wind moderate after a strong gale for two days from the SW.
25	37, 5	21,35	10256	59,	58,	58, 5			59,	30,5	SE by E	
26	34,54	22, 5	10270	60,	59,	59, 5			60,	30,1	SE	
27	33,14	21,59	10264	61,	60,	60,			63,	30,2		
28	31,58	22,24	10260	61,	60,	60,			63,	30,		
29	30,41	22,23		61,	58,	59, 5			64,	30,1		
Mar. 1	28,25	23, 5	10260	64,	61,	63,			65, 5	30,2	ESE	
2	26,36	23,15	10273	64, 5	63,	63, 8			66,	30,2	E by S	
3	23,30	23, 6		65,	63,	64, 5	68,	66,	67,	30,2		Hazy.
4	21,32	22,36		66, 5	65,	65, 8	68,	67,	67, 5	30,1		Cloudy.
5	19, 1	22,47	10256	66, 5	65,	65, 8			68,	30,1	ESE	
6	16,47	22,37	10267	67,	63,	65, 3			68, 5	30,2	NE by E	Fine.
7	15, 4	21,26	10276	68,75	67,	64, 6			71,	30,		
8	12,56	20,20	10275	69, 5	68, 5	68, 8			71, 5	29,9	ESE	
9	11, 8	19,		73,	71, 5	72, 3			74,75		NE by E, E by S	
10	9,42	19,20	10276	76,	73	74, 3			76,	29,9	E by S	
11	8,23	19,16	10277	76,	71,75	74,			77,	29,0	N by W to S by E	
12	6,57	19,10	10277	79, 5	77,	77, 3	79,25	77, 5	78, 6	29,9	NW	
13	5,50	18,41		80,	77,25	78, 6	80,75	79, 5	80, 4	29,8		Cloudy.
14	5, 4	18,50		79, 5	74,	76,	80, 5	78, 5	80,	29,	North	Thunder storms with heavy rain.
15	4, 9	19,15	10277	80,	78, 5	79,	81, 5	80,	80, 7	29,	S, SE by N	Pleasant.
16	4, 2	18,44	10275	82,	78, 5	79, 6	83, 5	80,	81, 8	29,4	SE by E	Calms and squalls with thunder storms and heavy rain in succession.
17	4, 0	18,30	10270	80,	74,	77,	81,	80, 5	80, 5	29,		
18	2,58	18,44	10270	77,75	76, 5	77, 5	79,75	79, 5	79, 5	29,	SE to E	
19	2,27	19, 2		80,	77,	78, 6	79,75	79,	79, 5	29,	SSE	Fine.
20	1,20	21,10	10264	80,	78,	79,	79,	79,	79,	29,	ESE	
21	S. 0,12	21,50	10264	79, 5	78,	78, 8	78,75	78,	78, 5	30,2	ESE	
22	1,28	22,20	10264	79, 5	78,	79,	79, 5	78, 5	79, 2	30,	SE by S	
23	2,29	23, 5		79,75	78,75	79,	79, 5	79,	79,25	30,	SSE	
24	4,13	23,15		80, 5	78,25	79, 3	80,	79,	79,75	30,	ES by S	
25	6,27	24,13		80, 5	79,	79, 6	80, 5	79,75	80,	30,		
26	8,46	24,21		80, 5	79,	79, 3	80,	79,	79,75	30,3	SE by E	
27	10,30	24,25	10263	80,	79,	79, 3	80,	79,75	79,75			
28	12,12	24,50		79,25	78,	78,	80,25	79, 5	80,	30,2	ESE	Cloudy, showers.
29	13,45	25, 7		80, 5	77,	79,	80, 5	80,	80,25	30,2	SE by E	
30	15,35	26,		81,	78,	79,	80,75	80,	80, 5	30,2	ESE	
31	17,42	27,		80,25	77,25	79,	80,75	77, 5	80,	30,2	SE by S, ENE	
Apr. 1	19,47	27,23		81,	78,	79, 4	81,	79, 5	80,	30,1	E by S	Very fine.
2	21, 3	27,27		80,	77, 5	78, 5	80,75	78, 5	80,	30,2		
3	22,36	26,30	10264	80,	77, 5	77, 7	80,25	79,	79,75	30,2	ENE	
4	23,44	26,29		80,	76,	77, 5	80, 5	79,	79,75	30,1		
5	24,22	26,27		79,75	76,	77,	80, 5	77,75	79,			

Time.	Latitude by Observation	Longitude by Chronometer.	Specific gravity of sea water at temp. 80.	Maximum temp. of the air in the 24 hours.	Minimum.	Mean.	Maximum temp. of the sea in the 24 hours.	Minimum.	Mean	Barometer.	Winds.	Weather, &c.
	° ″	° ″		° ″	° ″	° ″	° ″	° ″	° ″	° ″		
Apr. 6	S. 25,22	W26,33		80,	75, 5	77, 8	79, 5	77, 5	78, 2		NNE	
7	26,30	26,29		79,	73,	75,75	78, 5	76,75	77, 3	30, 5	NW by W, ESE	Unsettled, frequent showers, lightning.
8				73, 5	70,	72, 6	78,	75, 5	76, 8	30, 7	ESE to S	Tempestuous, sun obscured, heavy rain, a great swell from the SE.
9				73,	70,	71, 6	77,	72,	74,	31,	ESE	
10	27,50			71, 5	70,	70, 7	73,	72,	72, 4	30, 5		
11				71,	68,	69, 5	72, 5	71,75	72,	30, 3		
12	29,17	25,25		73,	70,	71, 5	72,	70,	71,	29, 8	NE by E	Pleasant. [night fine.
13	30,16	23,10		71,75	70,	71,	71, 5	69, 5	71,		N by E, NW	Variable, the day squally, the
14	30,20	23, 5		74,	70,	71, 6	74,	71,	72,			Calm.
15	30,25	20,	10256	73, 5	70, 5	71, 7	72, 5	71,	71, 7	30, 0	NE to NW	Pleasant.
16	31, 6	18,55		71,	66,	66, 5	71,25	67,	68, 6	29, 9	WSW, SSW	
17	31,48	17, 1		66,75	64,	65, 5	69,25	65,	68,	30, 7	SW by W	
18	32,12	16, 3		67,75	66,	67, 7	70,	68,	69,	30, 4	NW	Extremely damp, at night a little dew fell, the first observed.
19	33,29	15,17		71,	66,	68, 3	68, 5	65,	68,	30, 2	NNE	
20	33,58	10,48		67, 5	65,	66,	65,75	63,	65,	30, 1		
21	34,29	7,56		68,	63,	66,	66,25	65,	65, 7	30,		
22	34,26	5,30		64, 5	60,	62,	63,75	62, 5	63,	30, 1	SW	Air dryer, no dew.
23	34,25	3, 2	10253	64,	60,	62, 2	65,	63,75	64, 5	29, 7	W	Cloudy, some rain.
24	33,25	2,50		65,	61,	63, 5	66,	65,	65,25	29, 9	W by S	Cloudy.
25	33,43	0,43		63, 5	60, 5	61, 6	65,	63, 5	64, 2	30,	SW, SSE	
26	33,28	1, 2		62, 5	59,75	61,	64, 5	62,75	63, 6	30,	SW	
27	34,14	1, 1		61,	59,75	60, 3	64, 5	62, 5	63,75	30, 2	,	
28				63,	59, 5	62, 2	64, 5	62,	62, 6		E by N	Hazy.
29	35,33	2,13		64, 5	61,	62, 6	64,	62,	63,		W by N	Cloudy.
30	34,45	5,31	10251	65, 5	62, 5	63,75	64, 5	62, 5	63, 4			
May 1	34,36	7,13		66,	63,	64, 4	65,	61,75	64,	30, 1	NW	Fine.
2	34,32			64,	61,	64, 6	66, 6	63, 5	65, 2	30, 2	NW, SSE	
3	33,26	10,11		61,	59,	60, 5	63, 5	60,	62,	30, 3	SE by S	
4	34,32			60,	58,	58, 5	63,	61,	61, 9	30, 4		Cloudy.
5	35,22	11,23		60,	58,	58, 4	65,	61,75	62, 7	30, 5	SSE, SE by S	Fine.
6	34,28	12, 7		59,	58,	58, 9	65,	59,25	63, 4	30, 5	ESE, E by N	
7	35, 1	13,20		61, 5	58,	60,25	64,	60,	61, 6	30,		Cloudy.
8	34,23	14,17		63,	57, 5	60,25	64, 5	61,	63,	30, 3	NW, SW	Squally.
9	34, 1	15,31		58,	56, 5	57,	63, 5	62,	63,		SW by S	
10	34,24	16,45		62,	59,	60,70	62,75	61, 5	62,	30,7 to 29	NW, SSW	More clear and moderate.
11	34, 1	17,51	10259	60,	57,	58,	62, 5	57,	60,	30, 2	SWS	
12				58,	54,	56,	57,	55,	56, 2	30, 2		In sight of Table Mountain, and within soundings.
June 3			10251	56,	51,	54, 1	55,25	53,	55,			All day in soundings.
4				60,	55,	58, 4	62,	57,	60, 3		SSW	Still in sight of land, water still greenish,
5				62,	59,	59,75	65, 5	58,	63,		SSW	Out of sight of land, water blue.
6	34,15			62,	59,	60, 4	63, 5	62,75	63,		ENE	Pleasant.
7	34,53	15, 8		63, 5	60,	61,25	64,	61,	62, 6		NNE	
8	35, 1	16,42		61,	60,	60, 5	61, 5	59,75	60, 8			Cloudy.
9	36, 4	19,26		64,	59, 5	61, 6	66,	60, 5	60, 3		NW	Tempestuous.
10	35,57	24,	10253	62,75	58,	59, 1	71, 5	61,	66, 9		NW, SW	
11	35,36	27,19		58, 5	57,	57, 6	67, 5	61,	65,		SW	
12	35,50	28,30		64,	57,	62,	67,	64,	66, 3		NW	Improving.
13	35,53	30,53		64,	61,	62, 9	65,	63,	64, 2			Pleasant.
14	35,52	33,54		63,25	62,	62, 6	65,	63, 5	64, 4		SW, N by W	Fine.

Time.	Latitude by Observation.	Longitude by Chronometer.	Specific gravity of sea water at temp. 80.	Maximum temp. of the air in the 24 hours.	Minimum.	Mean.	Maximum temp. of the sea in the 24 hours.	Minimum.	Mean.	Barometer.	Winds.	Weather, &c.
	° ″	° ′		° ″	° ″	° ″	° ″	° ″	° ″	° ′		
June 15	S. 35,31	W 36,		65,	59,	61, 3	64, 5	63, 5	63, 9	30,3	W by N, SW	
16	24,23	37,56		62,	60,75	61, 5	65, 5	64,	65,	29,8	SE, NW	Cloudy.
17	34,44	40, 7		65,	58,	61, 6	65,	61,	64,	30,3	NW, SE by S	A thunder storm.
18	33,48	42, 3		60,	58, 5	59, 5	65,	62,	63, 4	30,6	E by S	Pleasant.
19	34 48	42,24		63,	59, 5	61,	64,	61, 5	63,	30 to 31	NE	Cloudy, tempestuous, showery.
20	34,54	45,17		63,	61,	62,	64,	62,	62, 7	30,1	N	Incessant rain and thunder and lightning.
21	35, 7	48,40		63,	58,	61,	62,	60,	60, 9	30,	WNW	Improving.
22	34,34	51,35		62,	57,	59, 8	62,	60,	60, 9	30,1	W	
23	33,36	55,16		60,	57,	58, 7	62,25	61,	61, 5	30,1	W by S	Moderate.
24	32, 4	58,18	10260	60,	57,25	58, 3	62,25	61, 5	62,	30,1	SW, SE	Pleasant.
25	31, 2	60,		59,	56, 5	57, 3	62,	60,75	61, 2	30,1	NNW	Gloomy.
26	30,39	60,33		59,	57,	57, 2	63,	61,	61, 8	30,3	E by S, ENE	
27	30,48	60,36		61, 5	59, 5	60, 6	61, 5	60,25	61, 3	30,3	NE	
28	31,23	63,17		63,	61, 5	62, 5	63,75	62,	62, 5	30,2	N by E	
29	31,20	64,34		63, 5	62,	63,	63,75	61,	62, 5	30,1		
30	30,53	65,38		67,	62, 5	63,	65, 5	63, 5	64, 5	30,4	N, W by N	Fair.
July 1	30,58	65,45		66,	61, 5	63, 6	65,	63,25	63, 6	30,2	SW	Gloomy, some rain.
2	29,23	66,17		62,	62,	60, 7	67,	64,	65, 5	30,3	SE	Fair.
3	27,18	66,30		64,	60,	62, 4	68,75	65,75	67, 3	30,4	E	
4	24,45	65,45		67,	64, 5	65, 8	70 75	68,	69, 4	30,3	E by S, E by N	
5	21,45	65,25	10259	69,	67,	68,	72,	70,	71,	30,4	SE	
6	19,53	63, 1		71,	68,	69,	74,	72,	72, 5	30,		Off Rodriguez.
7	19,44	62,50		71,	69,	69, 5	73, 5	71, 5	72, 3	30,2	S by W	At night in sight of the Isle of France.
17	19,15	57,11		72,75	71,	71, 6	74, 5	72,	73,	30,1	E by S	Fair, out of sight of land.
18	18,14	57,30		74, 0	72,	72, 3	74,	72, 5	74, 7	30,2	E	
19	17, 6	57, 3		74, 5	72,	73, 4	75,	72, 5	74,	30,2	E by S	
20	15,46	58,		74, 5	72, 5	73, 6	76,25	75,	75, 8	30,2	ESE	Showers.
21	14,22	58,48		75,	74, 5	74, 7	76, 5	75,	76,	30,2	E	Night squally.
22	12,17	59, 3		76, 5	73, 5	75, 1	77,	74,25	75, 6	30,2	E by S	Fair.
23	10,12	60,		77,	74,75	75, 1	77, 5	75,	76,25	30,		
24	7,54	60,23		77, 5	72,	74, 6	77, 5	75,	75,75	30,		—— night squally.
25	5,42	60,51		76,	75,	75, 4	77,	75, 5	76, 2	30,1	SE	Squalls with rain.
26	4, 6	61,45		78,	76,	77,	78,25	75,75	76, 5			
27	2,40	62,51		78,	76,25	77,	79, 5	78, 5	79,		E by N	Cloudy, squalls with rain.
28	1,40	63,35	10253	79, 5	77, 5	78,	81,	77,	79, 1		NE	Improving.
29	1,14	63,30		79,	75, 5	78,	80, 5	78,	79, 4			Rainy night.
30	0,19	65,11		78,25	77,	77, 8	81,	79,	80, 2		SE	Pleasant.
31	N 0,22	66,36		81,	73, 5	77, 5	82, 5	78,	80, 5		W by N	Day calm, night rainy.
Aug. 1	0,30	67,52		77,75	75,	77,	80, 5	79, 5	80,		S by W	Much rain.
2	0,26	68,58		78,	76,	77, 5	81,	80,	80, 4		S by E, S by W	Unsettled, some rain.
3	0,58	70, 6		81,	78, 5	79, 9	81, 5	80,	80, 7		SSW	Fine
4	1,27	71,51		80, 5	78,75	79, 9	81,75	80,	80, 7		S by W	
5	1,17	73,53		81,	79,	79, 5	81, 5	79,75	81,		S	
6	1,37	75,36		80, 5	78,	79, 9	81.75	79, 5	80, 6		SW	
7	2,10	76,37		82,	76,	78, 8	83, 5	80,	81, 7		SW by W	Part of the day calm.
8	2,34	77,25		81,	77, 5	79, 6	82,25	79,25	80, 7		N by W	Pleasant.
9	2,30	77,53		82,	75, 5	77,	84,	78, 5	80, 3			Calm.
10	3,22	79, 4		79,75	77,	77, 6	79,75	78,25	79,			Calm till 4 P. M.
11	5,17	79,42		79,75	76,25	77, 6	79,	76,	77, 5		NW	Frequent squalls.
12	6,24			77,75	75,75	76, 6	78,	76,	76, 9		WNW	Pleasant, in sight of Ceylon, in soundings.

In all the experiments on the density of sea-water, the results of which are recorded in the Journal, the water used was taken from the surface of the ocean, in a large clean bucket. The results introduced before we passed the equator the first time, were procured at sea; the remainder, from 0° 12″ south lat. to Ceylon, were obtained on land from experiments made on specimens of water preserved in well corked phials. In the experiments on board ship, as soon as the water was drawn, its temperature was ascertained, and then it was immediately weighed. The balance employed was not very delicate, for a very delicate instrument does not answer at sea, on account of the ship's motion; however, it was pretty readily affected by $\frac{1}{10}$ of a grain. The glass vessel in which the water was weighed, was such a one as is commonly used at home; its capacity was equal to about 300 grains. In the experiments on shore, the same vessel was used, but a different balance, one of a more delicate construction. I have chosen the temperature 80° FAHRENHEIT, for which I have calculated all the results, because it is nearly the mean annual temperature of this place, and nearly the mean at sea, in the intertropical regions.

The experiments made at sea I do not of course value so much, as those made on land: considered, however, merely as approximations to the truth, which I am sure they are, the results favour the general conclusion already formed by some philosophers, that the ocean resembles the atmosphere in being (cæteris paribus) of nearly the same specific gravity throughout.

And farther, they lead to the conclusion, that the slight

variations of specific gravity observed, do not regularly conform to the difference of temperature.

That the specific gravity of the water of the ocean, in all its parts, however remote, should be nearly the same, is easily explained; it is indeed what might be expected from theory. It is more difficult, it appears to me, to account for the slight variations; I may remark, they appeared to me greatest when the sea was rough and agitated; and once the specific gravity of the water seemed diminished by a heavy fall of rain, viz. in lat. 4° north, and in long. 18° 13″ west, where we experienced a quick succession of tropical squalls.

Whether there is a specific gravity peculiar to the water of each zone, as a modern traveller of high authority endeavours to prove, I am greatly in doubt. From my own experiments, in which I cannot but put some reliance, I feel much inclined to infer the contrary, and especially from those made on land, which I know to be perfectly accurate. Several of these agree in giving the same specific gravity to specimens of water taken from parts of the ocean very remote from each other; for instance, the water from lat. 0° 12″ south, and 22° 36″ south, and that from 34° 25″ south, and the water that washes the shores of Colombo.

For ascertaining the temperature of the air and of the water of the ocean, I used delicate pocket-thermometers, the bulbs of which projected about an inch from the ivory scale. In the experiments on the temperature of the ocean, the water was tried the instant it was drawn, before it was affected by the air. To find the temperature of the air, I always chose the coolest part of the ship on deck, and always put the in-

strument in the shade, and exposed it to the wind, taking care not to bring it near any surface that had the power of radiating much heat, circumstances, I need not remark, of importance to be attended to, and, in consequence of the neglect of which, the temperature at sea, in the intertropical regions, has by most observers been overrated.

During the greater part of the voyage, observations were made every two hours, on the temperature both of the air and of the water; and with the kind assistance of the mates of the ship, Messrs. SLEIGHT and POWELL, intelligent and obliging men, they were carried on during the night as well as the day.

I am not aware that the law of the diurnal variation of the temperature of the atmosphere at sea, has been described by any writer. From the numerous observations, which I had an opportunity of making, between and bordering on the tropics, it appeared to me perfectly regular at a great distance from land, when the weather was fine, and the wind steady. In these circumstances, I found the air at its maximum temperature precisely at noon, and at its minimum towards sunrise. I shall give in illustration of the fact two instances from my note book.

April 2d. S. lat. 21° 3″. W. long. 27° 27″. Wind E. by S.

Hour.	Temperature.
6 A. M.	78° ″
8	79
10	79,5
12	80
2 P. M.	79,25
4	79

Hour.	Temperature.
6 - -	78,5
8 - -	78
10 - - - -	78
12 - -	77,75
2 A. M. - -	77,75
4 - - -	77, 5
6 - -	77, 5

April 5th. S. lat. 24° 22″ W. long. 26° 27″. Wind ENE.

Hour.	Temperature.
6 A. M. - -	76° ″
8 - -	77,5
10 - - -	78,25
12 - -	79,75
2 P. M. - -	78, 5
4 - -	77,75
6 - - - -	77, 5
8 - -	77
10 - -	77
12 - - -	76, 5
2 A. M. - -	76, 5
4 - - -	76

Here we perceive the variation of the temperature of the air, following the course of the sun, pretty considerable whilst it is above the horizon, and very insignificant during the night; and this, I may remark, is a general fact at sea, and one of the principal features of difference between the temperature of the atmosphere over the land, and over the ocean.

The law of the regular variation of temperature, is frequently interrupted. Even in fine weather, when the air is not in motion, it is subject to interruption. During a calm, the variation of temperature is nearly the same as on land, the maximum degree of heat not being at noon precisely, but some time after, and for the same reason; because there is an accumulation of heat, and not only in the ship, but actually in the water itself, as I may show by noticing the temperature of the air and of the sea, during even a short calm, hardly of 24 hours duration.

August 7th. N. lat. 2° 10″ E. long. 76° 37″.

Hour.	Temp. of air.	Of the sea.
6 A. M.	78°,5	80°
8	79,5	81
10	80,5	81,5
12	82	82,5
2 P. M.	82,5	83,5
3	82	83,5
4	81,5	

But the law is more remarkably interrupted during storms and unsettled weather, as a couple of instances will be sufficient to prove.

March 17th. N. lat. 4°. W. long. 18° 30″

Hour.	Weather.	Temp. of air.	of the rain water.
3 A. M.	Clear	80	
11	Rain approaching	77	76
11° 30	Just passed	74	73
12	Cloudy	79	
1 P. M.	After a shower	76,5	76
4	- - -	75	74

March 27th. S. lat. 10° 30″. W. long. 24° 25″.

Hour.	Weather.	Temp. of air.
5 A. M.	Fair	79
6	Rain approaching	78
6 30	Raining heavily	75,5
7	Rain just ceased	76,5
8	Sunshine	79,25
9	Raining	76
10	Cloudy	79,5
12	Fair	80,5

The showers in each instance were accompanied by hard gusts of wind, and thunder and lightning. The rain-water, the temperature of which was ascertained, was collected in a glass as it ran from the awning.

The equatorial regions appear to be particularly subject to storms, violent rain, and electrical phenomena, the effect of which, in diminishing the temperature, seems to afford a natural explanation of the comparative coolness, both of the atmosphere and the ocean, that we experienced each time we passed the line.

The temperature of the sea, it has been asserted by some writers, is subject to little or no diurnal variation. That this remark is far from correct, is evident from the slightest inspection of the Meteorological Journal; it is an opinion that could be formed only from hypothetical views, ill founded. The fact, as the Journal exhibits, is, that the diurnal change of the temperature of the sea is very nearly as great as that of the incumbent atmosphere. From all the observations I could make, when the circumstances were most favourable to accurate results, when the weather was fine, the sea smooth, and the land at a great distance, it appeared to me,

that the maximum temperature is about three in the afternoon, and its minimum towards sunrise. I shall give a single example in detail.

April 5th. S. 24° 22″. W. long. 27° 8″.

Hour.	Temp. of the sea.
	° ″
8 A. M. - -	79,25
10 - - -	79 ,5
12 - - -	79, 5
2 P. M. - -	80
4 - - -	80, 5
6 - - -	80
8 - - -	79, 5
10 - - -	79
12 - - -	78, 5
2 A. M. - -	78
4 - - -	77,75
6 - - -	76

Like the atmosphere, the ocean is subject to irregularities of temperature. This fact is proved by the Journal in an ample manner. The causes which produce these irregularities may be divided, very generally, into three kinds, tempestuous weather, shoals, and currents.

Independent of other modes of operation, and they are various in tempestuous weather, superficial currents appear to be established in the course of the prevailing winds. If the wind be from a cold quarter, the temperature of this current is comparatively low, and *vice versâ*. This fact is manifest in the effect of the gales we experienced between the 7th and 12th of April, during which time, being south of the

equator, and the wind blowing from the south, the temperature of the sea was considerably reduced.

Where the sea is shallow, it is now a well established fact,* that the temperature of the water is comparatively low; an important circumstance, highly deserving the attention of the practical navigator; it may forewarn him of a bank in the darkness of night, when nothing else would indicate it, and put him on his guard when approaching low shores and shallows, time enough to avoid their dangers. In advancing towards the Cape of Good Hope, and in doubling that promontory, and in making Ceylon, I collected some observations on this subject, the results of which I shall now introduce. On making Table-bay, before land was to be seen, there was a decided fall of the temperature of the water, viz. from above 60 to 58, thus,

May 11th. S. lat 34° 1″. E. long. 17° 51″ at

8 A. M. the temp. of the water was	62,5
10 - - - -	62,5
12 - - - -	61,5
2 P. M. - - - -	61
5 - - - -	60
10 - - - -	58
12 - - - -	58
2 A. M. - - - -	58,5
4 Land in sight - - -	59
7 About 20 miles from land -	58
8 - - - -	57

* Observed by Dr. FRANKLIN, Mr. J. WILLIAMS, &c. See WILLIAMS's Thermometrical Navigation. Philadelphia 1790

10	- - - - -	56
12	- - - -	56
2 P. M.	- - -	55
4	- - -	56
8 In soundings	- - -	56,5
10	- - - -	56,5
12	- - - -	55
4 A. M.	- - - -	55
6	- - - -	56,5
8	- - - -	56,5

During these two days we were gradually approaching land, at the average rate of about two miles an hour. The observations were continued, till we were within about two miles of the shore. The observations I made on leaving the bay, corresponded with the foregoing, as nearly as could be expected, considering the track was not precisely the same, and the cold season more advanced.

			° ″
June 3d.	8 A. M.	Half a mile from land, temp. of water	53
	10	About three miles from land	54,25
	2 P. M.	Off Robin Island, nine miles from Cape Town, in ten fathoms water -	55,25
	4	- - - - - -	55,25
	12	- - - - -	54, 5
	2 A. M.	- - - -	54, 5
	8	- - - - -	57, 5
	10	- - - -	57
	12	- - - - -	60
	2 P. M.	- - - - -	61
	4	- - - - -	62

Before four in the afternoon we were out of sight of the Cape of Good Hope, and in deep water.

In approaching Ceylon, and particularly the southern shore of the island, where the mean annual temperature appears to be about 80°, little or no change of temperature could be expected on entering shallow water; yet we experienced a manifest change, a reduction of at least two degrees on coming into soundings. When we were in north latitude 5° 17″, and east longitude by chronometer 79° 42″, the temperature of the water began to fall; in the morning at eight, it was 78° 5″ and at ten at night it was 76° 5″. Next morning, land was discovered.

From the observations, in general, on the temperature of the water, recorded in the Journal, there is reason to believe, that during the whole voyage we were frequently encountering currents. Many of the results stated, are scarcely to be explained on any other hypothesis. When the temperature of the water became suddenly reduced, I inferred we were either in a current from the poles, or over some high ground in the bed of the ocean; and the former conclusion was almost constantly confirmed by other observations. And on the contrary, when the temperature of the water experienced a sudden increase, I inferred that we were in a current flowing from the equatorial regions. The only current we passed, that appears to me to require particular notice, is the well known one, that flows round the bank of Lagullas, from the south-east coast of Africa. It is marked in all charts, and it has been pretty minutely, and very scientifically described, and its course explained by Major Rennell,

but hitherto, I believe, no notice has been taken of its high temperature, or of the effect which I believe it has, in producing a curious phenomenon on the summit of the Table-mountain, not yet accounted for, viz. a dense covering of mist called the "table-cloth," which universally appears when the wind blows from the south-east. I shall copy from my notes, taken at the time, the observations I made in crossing this current.

June 10th. S. lat. 35° 57″. E. long. 24°.

Hour.	Temperature of the Sea.
6 A. M.	61
8	71, 5
10	70, 5
11	70
12	68
1 P. M.	68, 5
2	67, 5
4	68
5	67
6	66, 5
7	67
8	67
9	67
10	66,75
11	67
12	67
1 A. M.	67
2	67
3	61
4	61

Hour.	Temperature of the Sea.
5	64
6	66,75
7	66
9	67
10	67, 5
12	66
2 P. M.	67, 5
4	65, 5

Now, judging from the change of temperature, we appear to have suddenly passed from the bank of Lagullas into the current that flows round its borders. Major RENNELL, I believe, observes, that at the border of the bank, the current is strongest; the high temperature of the water there, at least ten degrees above the neighbouring sea, is readily accounted for on that idea. We appear to have continued in the current seventeen hours, the course the ship was going was nearly due east, her average rate 7.65 miles an hour, and hence, supposing we were sailing immediately across the stream, as probably we were, or very nearly, its width may be inferred to be about 130 miles; a distance little differing from that commonly assigned to it. Having traversed this current, we seem from the low temperature of the water for two hours, to have been passing a bank twelve miles wide, and then to have entered a second current running in the same direction as the first.

I have alluded to a connection between these currents and the covering of dense mist, that occasionally occurs on the Table-mountain, called the "Table-cloth." The connection is evident, and readily explained. The phenomenon only

presents itself when a cold wind blows, viz. the south-east. This wind must condense the aqueous vapour rising from the warm current, and carry it towards the land. During the short stay we made at the Cape, I once had an opportunity of seeing the mist advancing; it came rapidly over the surface of the sea, which it entirely concealed, whilst the air above was perfectly clear; it soon reached the land, spread along the coast gradually, ascended the mountain, and there remained almost stationary, enveloping the summit, sometimes encreasing and descending on the opposite side overhanging Cape Town, and sometimes diminishing and retreating. That it should remain so nearly stationary on the top of Table Hill, whilst the south-east wind continues, is not surprising, considering the height of this hill, 3582 feet above the level of the sea, its precipitous sides, and the extensive surface of its top; nor is it strange, that it should rarely descend, except when the wind blows hard, taking into account the situation of the ground beneath, sheltered and warm, and the site of a large town, from which a current of hot air must be constantly rising.

I cannot conclude, without insisting with Mr. JONATHAN WILLIAMS on the use of the thermometer at sea; if commonly employed, and the observations made with it recorded, a general knowledge might soon be obtained of the average temperature of all parts of the ocean, and a fund of curious and useful information might be collected, especially respecting currents and shoals, that to practical navigators could not fail of being highly serviceable.

In another letter, I propose communicating to you the

observations I have collected on the temperature of man and other animals in different climates. The experiments were made during my voyage, and during my stay at the Cape, and the Isle of France, and my residence at this place.

I remain, &c.

JOHN DAVY.

Colombo, Nov. 3, 1816.

XXII. *Observations on the genus Ocythoë of* Rafinesque, *with a description of a new species.* *By* William Elford Leach, *M. D. F. R. S.*

Read June 5, 1817.

PLINY, ALDROVANDUS, LISTER, RUMPHIUS, D'ARGENVILLE, BRUGUIERE, BOSC, CUVIER, and SHAW, have described a species of this genus, that is often found in the *Argonauta argo* (common paper-nautilus) and which they have regarded as its animal, since no other inhabitant has been observed in it.

Sir JOSEPH BANKS, and some other naturalists, have always entertained a contrary opinion, believing it to be no more than a parasitical inhabitant of the Argonaut's shell, and RAFINESQUE, (whose situation on the shores of the Mediterranean, has afforded him ample opportunities of studying this animal, and of observing its habits) has regarded it as a peculiar genus, allied to the *Polypus** of ARISTOTLE, residing parasitically in the above mentioned shell.

Dr. BLAINVILLE, ten months since, when speaking of the *Argonauta*, said, " animal unknown," and he has lately informed me, that he has written a long dissertation to prove, that the *Ocythoë* of RAFINESQUE, does not belong to the shell in which it is found.

The observations made by the late Mr. JOHN CRANCH, zoologist to the unfortunate Congo expedition, have cleared from my mind any doubts on the subject. In the gulf of

* Sepia octopodia LINNÉ.

Guinea, and afterwards on the voyage, he took (by means of a small net, (which was always suspended over the side of the vessel) several specimens of a new species of *Ocythoë*, which were swimming in a small *argonauta*, on the surface of the sea.

On the 13th of June, he placed two living specimens in a vessel of sea water; the animals very soon protruded their arms, and swam on and below the surface, having all the actions of the common *polypus* of our seas: by means of their suckers, they adhered firmly to any substance with which they came in contact, and when sticking to the sides of the basin, the shell might easily be withdrawn from the animals. They had the power of completely withdrawing within the shell, and of leaving it entirely. One individual quitted its shell, and lived several hours, swimming about, and showing no inclination to return into it; and others left the shells, as he was taking them up in the net. They changed colour, like other animals of the class cephalopoda; when at rest the colour was pale flesh-coloured, more or less speckled with purplish; the under parts of the arms were bluish grey; the suckers whitish.

The *Ocythoë* differs generically from the *polypus*, in having shorter arms, with pedunculated instead of simple suckers; the superior arms too are dilated into, or furnished with, a wing-like process on their interior extremities.

All the internal organs are essentially the same as in the *polypus*, although they are somewhat modified in their proportion; but as these differences may be the result of the contraction caused by the spirits, in which they are preserved, it may be more prudent not to dwell on them. Two cha-

racters, however, which I could not discover in the *polypus*, may be mentioned, namely, four oblong spots on the inside of the tube, resembling surfaces for the secretion of mucus; two inferior and lateral, and two superior, larger, and meeting anteriorly. On the rim of the sac, immediately above the branchiæ, on each side, is a small, short, fleshy tubercle, which fits into an excavation on the opposite side of the sac. This character, which, with slight modifications is common to this genus, to *loligo* and *sepia*, does not exist in the *polypus*.*

Although the superior arms are stated to perform such different functions from those of the *polypus*, yet they are supplied in the same manner, and from the same source with nerves. The muscles of these parts were in too contracted a state, to enable me to ascertain if they were in any degree different from those in the same parts of its kindred genus.

The general form of the body of this species of *ocythoë*, is the same as that of the common *polypus*, and it is covered by the same integuments, without any surface adapted either to adhere to, or to secrete, the shell in which it is found. The sexes differ as in the *polypus*.

Ocythoë Cranchii.

O. corpore purpureo-punctato, brachiis subtus cærulescente-griseis; superioribus membranâ spongeosâ pallidâ maculatâ.

The superior arms are generally attached to the side of the membranes (fig. 5. Pl. XII.); but in one specimen the membranes adhere only by their base, below the apex of the

* The rudiment of the bone, which occurs in the *polypus*, (as has been observed by Cuvier) is not to be found in the *ocythoë*.

arm, fig. 6. The membrane is subject to great variation in size and form, and is often different on the arms of the same individual.

One male only was sent home, all the others were females, which had placed their eggs in the spiral part of the shell.

One female, that had deposited all her eggs, withdrew completely within the shell, as in fig. 3; her body on one side had all the impressions of the shell, and the suckers on all the arms were diminished in size, as if from pressure.

Explanation of Plate XII.

Fig. 1. Ocythoë Cranchii sitting within the shell.

Fig. 2. The animal without the shell.

Fig. 3. One completely retracted within the shell.

Fig. 4. Ditto taken out of the shell, showing the impressions of the shell on the body.

Fig. 5. Left superior arm (common appearance) magnified.

Fig. 6. Right superior arm (variety) magnified.

Fig. 1.

Fig. 2.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 6.

XXIII. *The distinguishing characters between the ova of the Sepia, and those of the vermes testacea, that live in water, explained.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read June 5, 1817.

Linnæus was led into an error respecting the animal that forms the shell argonauta, by the circumstance of a species of sepia having been often found in this shell. This erroneous opinion has been adopted by many naturalists upon the Continent, even those conversant in comparative anatomy.

Whether the argonauta is really an internal shell, which I have asserted it to be, may possibly never be determined by direct proofs, as the animal belonging to it has not been met with. The present observations are confined to the question of the probability of its being formed by the species of sepia frequently found in it; and the materials of the present Paper, which are furnished from the specimens of natural history collected in the late expedition to the Congo, enable me to prove, in contradiction to such an opinion, that the ova of this particular species of sepia are not those of an animal of the order vermes testacea, that live in water.

The young of all oviparous animals, while contained in the ovum, must have their blood aerated through its coats, but in the vermes testacea, if the shell were formed in the ovum, the process of aerating the blood, must be very materially interfered with, for this reason, the covering or shell of the egg first drops off, and the young is hatched before the shell of the animal is formed; this I have seen take place in the eggs

of the garden snail, but in the testacea that live in water, the young requires some defence in the period, between the egg being hatched, and the young acquiring its shell, which is not necessary in those that live on land; for this purpose, the ova are enclosed in chambers of a particular kind.

This camerated nidus in the larger animals of this tribe, must be familiar to all naturalists, since specimens in a dried state, containing the young shells completely formed, are to be met with in collections of natural history; but I am not aware that all the purposes for which such a nidus is supplied by nature, have ever been explained.

I have been informed by a friend, who while in the East Indies saw the chank (a shell belonging to the same genus with the *voluta pyrum* of Linnæus,) shed its eggs, that the animal discharged a mass of mucus, adapted to the form of the lip of the shell, and several inches in length; this rope of eggs, enclosed in mucus at the end which is last disengaged, was of so adhesive a nature, that it became attached to the rock, or stone, on which the animal deposited it. As soon as the mucus came in contact with the salt water, it coagulated into a firm membranous structure, so that the eggs became enclosed in membranous chambers, and the nidus having one end fixed and the other loose, was moved by the waves, and the young in the eggs, had their blood aerated; when the young were hatched, they remained defended from the violence of the waves, till their shells had acquired strength.

What passes under the sea, few naturalists can be so fortunate as to have an opportunity of observing, and although what I have stated was communicated to me by an eye witness, it required confirmation, as well as an opportunity

of examining the nidus, before I could give it my assent. Since that time, I have procured from my friend Mr. LEE, the Botanist, of Hammersmith, a portion of a camerated nidus brought from South Carolina, containing shells of an univalve, not very different from the chanks of the East Indies. This nidus is represented in the annexed drawing. (Pl. XIII. fig. 7.)

I have also, which is still more satisfactory, seen the camerated nidus of the helix janthina. This animal not living at the bottom of the sea, like the vermes testacea in general, deposits its ova upon its own shell, if nothing else comes in its way; one of the specimens of the shell of the janthina, caught in the voyage to the Congo, fortunately has the ova so deposited, as will be seen in the annexed drawings made by Mr. BAUER, who was so pleased with the appearance the parts put on in the field of the microscope, that he was desirous of making a representation of them. (Pl. XIII. fig. 1, 2, 3, 4, 5, 6.)

In this instance, the ova are single, but in other tribes, several ova are contained in one chamber. In the land snail, the eggs have no such nidus. The following observations respecting them, were made in the year 1773, the first year that I was initiated in comparative anatomy, under Mr. HUNTER. He kept snails to ascertain their mode of breeding, and the notes that were made at the time in my own hand writing, I now copy.

August 5, 1773. A snail laid its eggs, and covered them over with earth; Mr. HUNTER took one out and examined it; the egg was round, its covering strong, and of a white colour, with a degree of transparency; it had no yelk; a small speck was observable with a magnifying glass in the transparent contents.

On the 9th no apparent change had taken place. On the 11th the speck had enlarged, but was too transparent to admit of its form being distinguished; upon moving the speck it fell out of its place.

On the 12th the embryo was indistinctly seen.

On the 15th the embryo filled $\frac{1}{4}$ part of the egg, but the different parts were still indistinct.

On the 18th the body of the embryo had become larger, and the covering thicker.

On the 19th, the coverings or shells of all the eggs were more or less dissolved, so much so that Mr. HUNTER thought all the eggs were rotting, and the whole brood of young would be lost.

On the 20th, the young were hatched, and the shells completely formed.

On the 23d, when the young snails were put in water, their bodies came out of the shell, as in full grown snails.

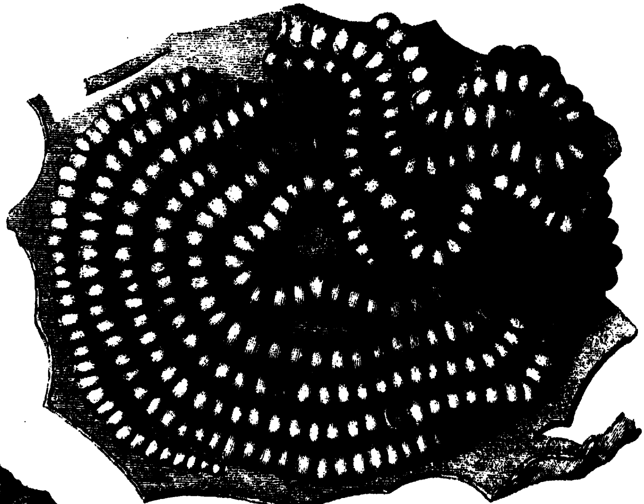
On the 24th, they all deserted their nests.

The specimens of the sepia found in the argonaut shell, which was caught by Mr. CRANCH, in this expedition to the Congo, had deposited some of its eggs in the involuted part of the shell, and the animal being fortunately caught in the shell, identified the eggs to belong to it; (Pl. XIV.) they are united together by pedicles, like the eggs of the sepia octopus, and in all other respects resemble them; they differ from those of the helix janthina and the other vermes testacea, that live in water, in having no camerated nidus, and in having a very large yelk to supply the young with nourishment, after they are hatched.

Upon these grounds, this animal must be resolved into a species of sepia, an animal which has no external shell,

Fig. 1.

Fig. 2.



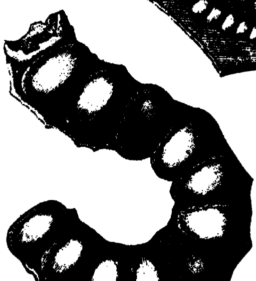


Fig. 3.

Fig. 4.

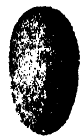


Fig. 5.

Fig. 6.

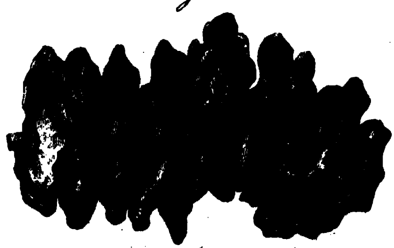


Fig. 7.

J. Basire sc.

Franz Bauer del. J. Basire sc.

and only uses the shell of the argonaut, when it occasionally gets possession of one.

Some naturalists, unacquainted with comparative anatomy, have asserted that in these eggs they saw the argonaut shell partly formed; they must have mistaken the yelk, which will be seen in the drawing to be unusually large, for the new shell.

Explanation of the Plates.

Plate XIII.

Fig. 1. The shell of the helix janthina, with the ová in its camerated nidus, attached to it; magnified twice in diameter.

Fig. 2. A portion of the nidus magnified 12 times in diameter.

Fig. 3. A string of the same nidus magnified 25 times in diameter.

Fig. 4. Two of the same ova and one empty chamber, magnified 50 times in diameter.

Fig. 5. One of the same ova, and

Fig. 6. The same slightly bruised, both magnified 50 times in diameter.

Fig. 7. A portion of the camerated nidus, in a dried state, belonging to the ova of a univalve from South Carolina, of the natural size.

Plate XIV.

Fig. 1. The shell of the argonauta, with the ova of the octopus deposited in it, magnified twice in diameter.

Fig. 2. A cluster of the same ova, as they are seen when immersed in water, magnified 12 times in diameter.

Fig. 3. One of the same ova with its pellicle, magnified 25 times in diameter.

Fig. 4. The yelk of the egg.

Fig. 5. A transversal section of the same.

Fig. 6. A longitudinal section of the same. The three preceding figures are magnified 50 times in diameter.

Fig. 7. A collapsed egg, as seen when taken out of the water, magnified 25 times in diameter.

XXIV. *Astronomical observations and experiments tending to investigate the local arrangement of the celestial bodies in space, and to determine the extent and condition of the Milky Way. By Sir* William Herschel, *Knt. Guelp. LL. D. F. R. S.*

Read June 19, 1817.

THE construction of the heavens, in which the real place of every celestial object in space is to be determined, can only be delineated with precision, when we have the situation of each heavenly body assigned in three dimensions, which in the case of the visible universe may be called length, breadth, and depth; or longitude, latitude, and Profundity.

The angular positions of the stars and other celestial objects, as they are given in astronomical catalogues, and represented upon globes, or laid down in maps, enable us, in a clear night, to find them by the eye or to view them in a telescope; for, in order to direct an instrument to them, a superficial place consisting of only two dimensions is sufficient; but although the line in which they are to be seen is thus pointed out to us, their distance from the eye in that line remains unknown; and unless a proper method for obtaining the profundity of objects can be found, their longitude and latitude will not enable us to assign their local arrangement in space.

With regard to objects comparatively very near to us, astronomers have completely succeeded by the method of parallaxes. The distance of the sun; the dimensions of the

orbits of the planets and of their satellites; the diameters of the sun, the moon, and the rest of the bodies belonging to the solar system, as well as the distances of comets, have all been successfully ascertained. The parallax of the fixed stars has also been an object of attention; and although we have hitherto had no satisfactory result from the investigation, the attempt has at least so far succeeded as to give us a most magnificent idea of the vast expansion of the sidereal heavens, by showing that probably the whole diameter of the earth's orbit, at the distance of a star of the first magnitude, does not subtend an angle of more than a single second of a degree, if indeed it should amount to so much; with regard to more remote objects, however, such as the stars of smaller size, highly compressed clusters of stars and nebulæ, the parallactic method can give us no assistance.

I. *Of the local situation of the stars of the heavens.*

The superficial situation of the stars having already been carefully assigned in the catalogues of astronomers, it will be proper to examine how far the arrangement of the stars into a certain order of magnitudes can assist us to determine their local situation.

When we look at the heavens in a clear night, and observe the different lustre of the stars, we are impressed with a certain idea of their different magnitudes; and when our estimation is confined to their appearance only, we shall be justified in saying, for instance, that Arcturus is larger than Aldebaran; the principle on which the stars are classed is, therefore, entirely founded on their apparent magnitude, or

brightness. Now, as it was thought convenient to arrange all the stars which in fine weather may be seen by the eye into seven classes, the brightest were called of the first, and the rest according to their gradually diminishing lustre, of the 2d, 3d, 4th, 5th, 6th, and 7th magnitudes. Then, since it is evident that we cannot mean to affirm that the stars of the 5th, 6th, and 7th magnitudes are really smaller than those of the 1st, 2d, or 3d, we must ascribe the cause of the difference in the apparent magnitudes of the stars to a difference in their relative distances from us; and on account of the great number of stars contained in each class, we must also allow that the stars of each succeeding magnitude, beginning from the first, are one with another farther from us than those of the magnitude immediately preceding. It may therefore be said, that since in our catalogues the magnitudes are added to the two dimensions which give the superficial place of the stars, we have also at least a presumptive value of the third dimension; but admitting that the naked eye can see stars as far from us as those of the seventh magnitude, this presumptive value, which can only point out their relative situation, will afford us no information about the real distance at which they are placed.

II. *Of a standard by which the relative arrangement of the stars may be examined.*

It is evident, that when we propose to examine how the stars of the heavens are arranged, we ought to have a certain standard of reference; and this I believe may be had by comparing their distribution to a certain properly modified equality of scattering. Now, the equality I shall here propose,

does not require that the stars should be at equal distances from each other; nor is it necessary that all those of the same nominal magnitude should be equally distant from us. It consists in allotting a certain equal portion of space to every star, in consequence of which we may calculate how many stars any given extent of space should contain. This definition of equal scattering agrees so far with observation, that it admits, for instance, Sirius, Arcturus, and Aldebaran to be put into the same class, notwithstanding their very different lustre will not allow us to suppose them to be at equal distances from us; but its chief advantage will be, that instead of the order of magnitudes into which our catalogues have arranged the stars, it will give us an order of distances, which may be used for ascertaining the local distribution of the heavenly bodies in space.

To explain this arrangement, let a circle be drawn with any given radius about the point S fig. 1, Plate XV. and with 3, 5, 7, 9, &c. times the same radius draw circles, or circular arcs, about the same centre. Then if a portion of space equal to the solid contents of a sphere, represented by the circle S, be allotted to each star, the circles, or circular arcs drawn about it will denote spheres containing the stars of their own order, and of all the orders belonging to the included spheres, and on the supposition of an equality of scattering, the number of stars of any given order may be had by inspection of the figure, which contains all the numbers that are required for the purpose; for those in front of the diagram express the diameters of spherical figures. The first row of numbers enclosed between the successive arcs, are the cubes of the diameters; the next column expresses the order

of the central distances; and the last gives the difference between the cube numbers of any order and the cube of the next enclosed order.

The use to be made of these columns of numbers is by inspection to determine how many stars of any particular order there ought to be if the stars were equally scattered. For instance, let it be required how many stars there should be of the 4th order. Then No. 4, in the column of the orders points out a sphere of nine times the diameter of the central one, and shows that it would contain 729 stars; but as this sphere includes all the stars of the 3d, 2d, and 1st order as well as the sun, their number will be the sum of all the stars contained in the next inferior sphere amounting to 343; which being taken from 729 leaves 386 for the space allotted to those of the 4th order of distances.

III. *Comparison of the order of magnitudes with the order of distances.*

With a view to throw some light upon the question, in what manner the stars are scattered in space, we may now compare their magnitudes, as we find them assigned in Mr. Bode's extensive catalogue of stars, with the order of their distances which has been explained.

The catalogue I have mentioned contains 17 stars of the 1st magnitude; but in my figure of the order of the distances we find their number to be 26.

The same catalogue has 57 stars of the 2d magnitude; but the order of distances admits 98.

Of the third magnitude the catalogue has 206, and the order of distances will admit 218.

The number of the stars of the 4th magnitude is by the catalogue 454, and by the order of distances 386.

Before I proceed, it may be proper to remark, that, by these four classifications of the stars into magnitudes, it appears already, that, on account of the great difference in the lustre of the brightest stars, many of them have been put back into the second class; and that the same visible excess of light has also occasioned many of the stars of the next degree of brightness to be put into the third class; but the principle of the visibility of the difference in brightness would have less influence with the gradually diminishing lustre of the stars, so that the number of those of the third magnitude would come nearly up to those of the third distance. And as the difference in the light of small stars is less visible than in the large ones, we find that the catalogue has admitted a greater number of stars of the 4th magnitude than the 4th order of distances points out; this may, however, be owing to taking in the stars that were thrown back from the preceding orders; and a remarkable coincidence of numbers seems to confirm this account of the arrangement of the stars into magnitudes. For the total number of the catalogued stars of the 1st, 2d, 3d, and 4th magnitudes, with the addition of the sun, is 735; and the number contained in the whole sphere of the 4th distance is 729.

Now the distinguishable difference of brightness becoming gradually less as the stars are smaller, the effect of the principle of classification will be, as indeed we find it in the 5th, 6th, and 7th classes, that fainter stars must be admitted into them than the order of distances points out.

The catalogue contains 1161 stars of the 5th magnitude, whereas the 5th order of distances has only room for 602.

Of the 6th magnitude the catalogue contains not less than 6103 stars, but the 6th order of distances will admit only 866.

And lastly, the same catalogue points out 6146 stars of the 7th magnitude, while the number of stars that can be taken into the 7th order of distances is only 1178.

The result of this comparison therefore is, that if the order of magnitudes could indicate the distance of the stars, it would denote at first a gradual, and afterwards a very abrupt condensation of them; but that, considering the principle on which the stars are classed, their arrangement into magnitudes can only apply to certain relative distances, and show that taking the stars of each class one with another, those of the succeeding magnitudes are farther from us than the stars of the preceding order.

IV. *Of a criterion for ascertaining the Profundity, or local situation of celestial objects in space.*

It has been shown that the presumptive distances of the stars pointed out by their magnitudes can give us no information of their real situation in space. The statement, however, that one with another the faintest stars are at the greatest distance from us, seems to me so forcible, that I believe it may serve for the foundation of an experimental investigation.

It will be admitted, that the light of a star is inversely as the square of its distance; if therefore we can find a method

by which the degree of light of any given star may be ascertained, its distance will become a subject of calculation. But in order to draw valid consequences from experiments made upon the brightness of different stars, we shall be obliged to admit, that one with another the stars are of a certain physical generic size and brightness, still allowing that all such deviations may exist, as generally take place among the individuals belonging to the same species.

There may be some difference in the intrinsic brightness of starlight: that of highly coloured stars may differ from the light of the bluish white ones; but in remarkable cases allowances may be made.

With regard to size, or diameter, we are perhaps more liable to error; but the extensive catalogue which has already been consulted, contains not less than 14,144 stars of the seven magnitudes that have been adverted to; it may therefore be presumed that any star promiscuously chosen for an experiment, out of such a number, is not likely to differ much from a certain mean size of them all.

At all events it will be certain that those stars the light of which we can experimentally prove to be $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{25}$, $\frac{1}{36}$, and $\frac{1}{49}$ of the light of any certain star of the 1st magnitude, must be 2, 3, 4, 5, 6, and 7 times as far from us as the standard star, provided the condition of the stars should come up to the supposed mean state of diameter and lustre of the standard star, and of this, when many equalisations are made, there is at least a great probability in favour.

V. *Of the equalisation of starlight.*

In my sweeps of the heavens, the idea of ascertaining the

Profundity of space to which our telescopes might reach, gave rise to an investigation of their space penetrating power; and finding that this might be calculated with reference to the extent of the same power of which the unassisted eye is capable, there always remained a desideratum of some sure method by which this might be ascertained.

Of various experiments I have long ago tried, the equalisation of starlight, which about four years ago I began to put into execution, appeared to be the most practicable. A description of the apparatus and the method of making use of it is as follows.

Of ten highly finished mirrors I selected two of an equal diameter and focal length, and placed them in two similarly fitted up seven feet telescopes. When they were completely adjusted, I directed them both, with a magnifying power of 118, to the same star, for instance, Arcturus: and upon trial I found the light not only of this, but of every other star to which they were directed, perfectly equal in both telescopes.

The two instruments, when I viewed the stars, were placed one a little before the other, and so near together that it would require little more than one second of time to look from one into the other. This convenient situation of the instruments is of great importance. The impression of the light made by the view of one star should be succeeded as soon as possible by the view of the other; and these alternate inspections should also be many times repeated, in order to take away some little advantage which the last view of a bright object has over that immediately preceding.

In comparing the light of one star with that of another, I laid it down as a principle, that no estimation but that of per-

fect equality should be admitted; and as the equal action of the instruments was now ascertained, I calculated the diameters of several apertures to be given to one of the telescopes as a standard, so that the other, called the equalising telescope, might be employed, with all its aperture unconfined, to examine a variety of stars, till one of them was found whose light was equal to that of the star to which the standard telescope was directed.*

In order to be sufficiently accurate in the calculation of the diameter of the limiting apertures, I thought it necessary to take into consideration not only the obstruction of incident light occasioned by the interposition of the small mirror, but also of the arm to which it is fastened, and proceeded as follows:

If A be the diameter of the large mirror; b that of the small one; $\frac{A-b}{2}$ the length of the arm; t its thickness; π the circumference, diameter being unity; x an assumed quantity for finding the correction; A′ the aperture corrected for the interposition of the arm; L the light of the equalising telescope; p the proportion of the light required for the standard telescope; D the diameter of an aperture to give that light; D′ the diameter corrected for the interposition of the arm.

Then will the diameters of the limiting apertures be had by the following equations. $\frac{A-b}{2} \times t = \pi A x$; $\frac{A-b}{\pi A} \times t = 2x$; $A - 2x = A'$; $A'^2 - b^2 = L$; $pL = D^2 - b^2$; $\sqrt{pL + b^2} = D$; $\frac{D-b}{\pi D} \times t = 2y$; $D + 2y = D'$ the required diameter.

* I preferred the limitation of the light by circular apertures to the method of obtaining it by the approach or recess of two opposite rectangular plates, in order to avoid the inflections which take place in the angles.

In the calculation of a set of apertures for the intended purpose, I admitted none that gave less than $\frac{1}{4}$ of the light; for by a greater contraction of the aperture of the mirror, an increase of the spurious diameters would render a judgment of equality liable to deception;† when therefore a star of the third order of distances was to be found, I rejected the direct way of reducing a star of the first order to $\frac{1}{9}$ of its light, but selected a star previously ascertained to be of the second order; and by taking $\frac{4}{9}$ of its light, the equalising telescope, with all its light, was used to examine all such stars as appeared likely to give the required equality, till one of them was found; nor was it necessary to have a great number of limiting apertures, as it soon appeared that with eight or ten of them I could have many different gradations of light, which would ascertain even fractional degrees, and reach as far as the stars of any order of distances I could expect to be visible to the unassisted eye.

This method of equalising the light of the stars, easy as it may appear, is nevertheless subject to great difficulties; for as the brightness of a star is affected by its situation, with regard to the ambient light of the heavens, the stars to be equalised should, if possible, be in nearly the same region. When the sun is deep under the horizon, this is however not of so much consequence as the altitude of the star to be equa-

† This was fully proved by the following experiment. July 27, 1813, I viewed Arcturus in a 10 feet reflector; first with all its light; next with circular diaphragms, which confined its aperture to $\frac{1}{4}$, $\frac{1}{9}$, $\frac{1}{16}$, $\frac{1}{25}$, $\frac{1}{36}$, and $\frac{1}{49}$ of it; but I found that the different spurious diameters, arising from the smallness of the apertures, made estimations of what is generally called the magnitude of the stars impossible.

See also experiments on the spurious diameters of the celestial objects, Phil. Trans. for 1805, page 40.

lised, which ought to be as nearly as possible equal to that of the standard star. At great elevations some difference in the altitudes of the stars to be equalised may be admitted; but, if they are far from each other, the circumstance of the equal illumination of the heavens, and the equal clearness of the air, must still be attended to.

VI. *Of the extent of natural vision.*

The method of equalising star light may be rigorously applied to ascertain the extent of natural vision; for in this case it will not be required that the star on which the experiment is tried, should be of the same size or diameter with the standard star; nor is it necessary that the intrinsic brightness of the light of the two stars should be the same in both. It will be sufficient, that the star we choose for an equalisation is one of the smallest that are still visible to the natural eye. It is also to be understood that, till we can have a well ascertained value of the parallax of any one star of the first magnitude, the extent of natural vision can only be given in a measure of which the distance of the standard star is the unit.

The following equalisations were made in August and December 1803, and February 1814, and are given as a specimen of the method I have pursued.

Taking Arcturus for the standard of an experiment, I directed the telescope, with one quarter of its light, upon it; while the equalising telescope, with all its light, was successively set upon such stars as I supposed might be at double the distance of the standard star; which, as Arcturus is a star

of the first magnitude, I expected to find among those of the second.

The first I tried was β (FL. 53) Pegasi, but I found it not quite bright enough.

The light of α Andromedæ, which next I tried, was nearly equalised to that of Arcturus; and the observation being repeated on a different night gave it equal.

Now as in these experiments the standard star is supposed to be one of the first order of distances, it follows that, if Arcturus were put at twice its distance from us, it would then appear like α Andromedæ, as a star of the 2d magnitude, and would also at the same time be really a star of the 2d order of distances.

In order to obtain some other stars whose light might be equalised by one quarter the light of Arcturus, I tried many different ones, and found among them α Polaris, γ Ursæ, and δ Cassiopeæ. These stars therefore may also be put into the class of those whose light is equal to the stars of the second order of the distance of Arcturus.

For the purpose of ascertaining the extent of natural vision, it will not be necessary here to give the equalisation of stars of the 3d, 5th, 6th, or 7th order of distances; but taking now the light of one of the stars of the 2d order of distances for a standard, I tried many that might be expected to have the required light, and found that when α Andromedæ, with its light reduced to one quarter, was in the standard telescope, the equalising one gave μ (FL. 48.) Pegasi for a star of the 4th order of distances. That is to say, the equalisation proved that, if Arcturus were placed at four times its distance from

us, we should see it as a star of the 4th magnitude, and also as one of the 4th order of distances.

Proceeding in the same manner with *μ* Pegasi taken as a standard, I found that its light reduced to $\frac{1}{4}$ was equal to that of *q* (FL. 70.) Pegasi, when seen in the equalising telescope; and that consequently Arcturus, removed to 8 times its present distance from us, would put on the appearance of a star which in our catalogues is called of the 5th or 6th magnitude, but which would in fact be of the 8th order of distances.

As the foregoing experiments can only show that a star of the light of Arcturus might be removed to 8 times its distance, and still remain visible to the naked eye as a star of between the 5th and 6th magnitude; it will be proper to take also other stars of the first magnitude for the original standards.

For instance, if we begin from Capella as the standard star, we may with $\frac{1}{4}$ of its light equalise *β* Aurigæ and *β* Tauri, which stars will therefore be of the 2d order of distances. With $\frac{1}{4}$ of the light of *β* Tauri we equalise *ζ* Tauri and *ι* Aurigæ; they will then be of the 4th order. With $\frac{1}{4}$ of the light of *ι* Aurigæ we can equalise *e* Persei and H Geminorum which will be of the 8th order. And with $\frac{16}{25}$ of the light of H Geminorum we equalise *d* Geminorum, which makes it a star of the 10th order. That is to say, if Capella were successively removed to 2, 4, 8 and 10 times the distance at which it is from us, it would then have the appearance of the stars which have been named.

A similar deduction may be made from *α* Lyræ, as $\frac{1}{4}$ of its light equalises it with *β* Tauri; for it will be *α* Lyræ 1, *β* Tauri 2, *ι* Aurigæ 4, H Geminorum 8, and *d* Geminorum 10: the

numbers annexed to the stars expressing their orders of distances in terms of the distance of α Lyræ from us.

To find stars of the intermediate orders of distances, the following Table gives the proportional light that should be used with the star which is made the standard; for instance, a star of the 2d order of distances, with $\frac{4}{9}$ of its light, will equalise a star of the 3d order; $\frac{9}{25}$ of the light of a star of the 3d order of distances will give one of the 5th order, and so on.

A star of the order of distances.	With the proportion of its light.	Gives one of the order of distances.
1 . .	. . $\frac{1}{4}$ . .	. . 2
2 . .	. . $\frac{4}{9}$ . .	. . 3
	. . $\frac{1}{4}$ . .	. . 4
3 . .	. . $\frac{9}{25}$ . .	. . 5
	. . $\frac{1}{4}$ . .	. . 6
4 . .	. . $\frac{16}{49}$ . .	. . 7
	. . $\frac{1}{4}$ . .	. . 8
5 . .	. . $\frac{25}{81}$ . .	. . 9
	. . $\frac{1}{4}$ . .	. . 10
6 . .	. . $\frac{36}{121}$ . .	. . 11
	. . $\frac{1}{4}$ . .	. . 12

Some other proportions of light useful for fractional distances are $\frac{9}{16}$, $\frac{16}{25}$, $\frac{36}{49}$, $\frac{64}{81}$, and $\frac{100}{121}$.

The results of equalisations that are made with different standard stars, may be connected together by an equalisation of the standards; by which means many different sets may be brought to support each other. For instance, Capella with $\frac{36}{49}$ of its light is of an equal lustre with Procyon, which therefore is of the $1\frac{1}{6}$ order of Capella, and Sirius with

$\frac{1}{49}$ of its light is also of an equal lustre with Procyon, which consequently, with regard to Sirius, is of the $1\frac{3}{4}$ order; then, by compounding, it follows that Capella to Sirius is a star of the $1\frac{1}{2}$ order, and from this we obtain the following series. Sirius 1, Capella $1\frac{1}{2}$, Procyon $1\frac{3}{4}$, β Tauri 3, ι Aurigæ 6, H Geminorum 12, and *d* Geminorum 15. By this connection we shall be able to obtain an equalisation of the same ultimate star with all the standards; for if Sirius must be removed to the 15th order, to appear as faint as *d* Geminorum; and if Capella, and also α Lyræ must be removed to the 10th order of distances to appear as faint as the same star, then any star of the size and brightness of Sirius, Capella, and α Lyræ must generally appear as faint as *d* Geminorum, when it is removed to nearly 12 times its distance; and the more stars of the first order are admitted in these general equalisations reduced to the same faint star, the more will the probability of the result be extended. Now as *d* Geminorum is a star of the 6th magnitude, we may expect that a still fainter visible star will give a somewhat greater extent to the reach of the natural eye, if however I take its vision, including other stars of the 1st magnitude, to extend to the 12th order of distances, there will probably be no material error, at least none but what a diligent astronomer, who is provided with the necessary apparatus, may correct by observation.

But the extent of natural vision is not limited to the light of solitary stars only; the united lustre of a number of them will become visible when the stars themselves cannot be seen. For instance, the milky way; the bright spot in the sword handle of Perseus; the cluster north of η and H Geminorum; the cluster south of Fl. 6 and 9 Aquilæ; the clus-

ter south of η Herculis, and the cluster north preceding ϵ Pegasi. But their distances cannot be ascertained by the method of equalising starlight: their probable situation in space may however be deduced from telescopic observations.

To these very faintly visible objects may be added two of a different nature, namely, the nebulosity in the sword of Orion, and that in the girdle of Andromeda.

VII. *Of the extent of telescopic vision.*

The powers of telescopes to penetrate into the Profundity of space is the result of the quantity of light they collect and send to the eye in a state fit for vision. The method of calculating the quantity of this power has been fully explained in a Paper read before the Royal Society, November 21, 1799; and the formulæ which have been given in that Paper have already been applied to show to what extent this power has been carried in the telescopes I used for astronomical observations. The calculated results, however, give this power only in reference to that of natural vision, and the uncertainty in which we were left with regard to its extent, was equally thrown over that of telescopic vision.

The equalisation of starlight, when carried to a proper degree of accuracy, will do away the cause of the error to which the telescopic extent of vision has been unavoidably subject, we may therefore safely apply this vision to measure the Profundity of sidereal objects that are far beyond the reach of the natural eye; but for this purpose the powers of penetrating into space of the telescopes that are to be used must be reduced to what may be called gaging powers; and as

the formula $\frac{\sqrt{x \cdot A^2 - b^2}}{a}$* gives the whole quantity of the space penetrating power, a reduction to any inferior power p, may be made by the expression $\sqrt{\frac{p^2 a^2}{x} + b^2} = A$; when the aperture is then limited to the calculated value of A, the telescopes will have the required gaging power. Or we may prepare a regular set of apertures to serve for trials, and find the gaging powers they give to the telescope by the original formula.

In the formula by which the required apertures for the gaging powers were calculated, a has been put equal to two tenths of an inch, and to show that this assumption is founded upon observation, I give the following extract from my astronomical Journal.

Dec. 27, 1801. I looked at α Lyræ with one eye shut and the other guarded by a slip of brass with holes of various sizes in it. Through the hole which was 0.28 inch in diameter, I saw the star just as well as without the limiting diaphragm, which shows that the opening of the pupil of the eye does not exceed 0.28 inch.

I tried the same star through 0.24 and still saw it equally well. I tried next 0.21 and still saw it as well.

The slip of brass was held as close to the eye as possible. The next I tried was 0.17 in diameter, and through this I could perceive a small deficiency of light, so that the opening of the pupil exceeds 0.17 inch. The night is hardly dark enough yet for great accuracy.

Having been out long in the dark, and trying the same

* See Phil. Trans. for 1800, page 66.

experiment upon many different large and small stars, they all concur to show that 0.21 does not sensibly stop any light; but that less does certainly render the object rather less luminous; so that the opening may be put at two-tenths of an inch in my eye.

VIII. *Application of the extent of natural and telescopic vision to the probable arrangement of the celestial bodies in space.*

When the extent of natural and telescopic vision is to be applied to investigate the distance of celestial objects, the result can only have a high degree of probability; for it will then be necessary to admit a certain physical generic size and brightness of the stars. But when two hypotheses are proposed to explain a certain phenomenon, that which will most naturally account for it ought to be preferred as being the most probable. Now as the different magnitudes of the stars may be ascribed to a physical difference in their size and lustre, and may also be owing to the greater distance of the fainter ones, we cannot think it probable that all those of the 5th, 6th and 7th magnitude, should be gradually of a smaller physical construction than those of the 1st, 2d, and 3d; but shall, on the contrary, be fairly justified in concluding that, in conformity with all the phenomena of vision, the greater faintness of those stars is owing to their greater distance from us. The average size and brightness of several stars of the first magnitude being also taken as a standard, in the manner that has been shown, the conclusion drawn from different series of equalisations will support one another; so that we shall be able to say a distant celestial object is so far from us,

provided the stars of which it is composed are of a size and lustre equal to the size and lustre of such stars as Sirius, Arcturus, Capella, Lyra, Rigel, and Procyon, &c.

I proceed now to consider some conclusions that may be drawn from a known extent of natural vision, a very obvious one of which is, that all the visible stars are probably contained within a sphere of the 12th order of distances. Now as on the principle of equal scattering, we should see about 15625 of them, it may be remarked that the stars of the catalogue, including all those of the 7th magnitude, amount to 14144, which agrees sufficiently well with the calculated number; but the next inference is, that if they were equally scattered, there would be 2402 of the 10th, 2906 of the 11th, and 3458 of the 12th order of distances, which added together amount only to 8766, whereas the number of stars of the 6th and 7th magnitudes that must come into these three orders, is not less than 12249, which would indicate that the stars in the higher order of distances are more compressed than they are in the neighbourhood of the sun; but from astronomical observations, we also know that the stars of the 6th and 7th magnitude are very sparingly scattered over many of the constellations, and that consequently the stars which belong to the 10th, 11th, and 12th order of distances, are not only more compressed than those in the neighbourhood of the sun, but that moreover their compression in different parts of the heavens must be very unequal.

IX. *Of the construction and extent of the milky way.*

Of all the celestial objects consisting of stars not visible to the eye, the milky way is the most striking; its general

appearance, without applying a telescope to it, is that of a zone surrounding our situation in the solar system, in the shape of a succession of differently condensed patches of brightness, intermixed with others of a fainter tinge.

To enumerate a partial series of them, we have a very bright patch under the arrow of Sagittarius; another in the Scutum Sobiescii; between these two there are three unequally bright places; north preceding α β and γ Aquilæ is a bright patch; between Aquila and the Scutum are two very faint places; a long faint place follows the shoulder of Ophiucus; near β Cygni is a bright place; near γ is another, and a third near α. A smaller brightish place follows in the succession of the milky way, and a large one towards Cassiopea. A faint place is on one side; a second towards Cassiopea, and a third is within that constellation; a very bright place is in the sword handle of Perseus; and α and γ Cassiopeæ inclose a dark spot.

The breadth of the milky way appears to be very unequal. In a few places it does not exceed five degrees; but in several constellations it is extended from ten to sixteen. In its course it runs nearly 120 degrees in a divided clustering stream, of which the two branches between Serpentarius and Antinous are expanded over more than 22 degrees.

That the sun is within its plane may be seen by an observer in the latitude of about 60 degrees; for when at 100 degrees of right ascension the milky way is in the east, it will at the same time be in the west at 280; while in its meridional situation it will pass through Cassiopea in the Zenith, and through the constellation of the cross in the Nadir.

From this survey of the milky way by the eye I shall now

proceed to show what appears to be its construction by applying to it the extent of telescopic vision; but as I had prepared a gradually increasing series of reductions of the space-penetrating powers of my instruments for the purpose of measuring the Profundity of sidereal objects not visible to the eye, which I have called gaging powers, it will be necessary to give the following account of it.

From the formula which has been given, I calculated a set of apertures, which by limiting the light of the finder of my seven feet reflector would reduce its space-penetrating power to the low gaging powers 2, 3, and 4. I then limited in the same manner the space-penetrating power of my night glass, by using calculated apertures such as would give the gaging powers 5, 6, 7 and 8. From the space-penetrating power of the 7 feet reflector, I obtained by limitation the successive gaging powers 9, 10 and upwards to 17. And lastly, by limiting the space-penetrating power of my 10 feet reflector, I carried the gaging powers from 17 to 28.

For the purpose of trying these powers, I selected the bright spot in the sword handle of Perseus, as being probably a protuberant part of the milky way, in which it is situated. Its altitude at the time of observation was about 30 degrees, and no star in it was visible.

In the finder with the gaging power 2, I saw many stars; and admitting the eye to reach to stars at the distance of the 12th order, we may conclude that the small stars which were visible with this low power, are such as contribute to the brightness of the spot, and that their situation is probably from between the 12th to the 24th order of distances; at least we are certain, that if stars of the size and lustre of

Sirius, Arcturus, Capella, &c. were removed into the Profundity of space which I have mentioned, they would then appear like the stars I saw with the gaging power of the finder. I then changed this power from 2 to 3, and saw more stars than before; and changing it again from 3 to 4, a still greater number of them became visible. The situation of these additional stars was consequently between the 24th, 36th, and 48th order of distances.

With the gaging power 5 of the night glass I saw a great number of stars; with 6, more stars and whitishness became visible; with 7, more stars with resolvable whitishness were seen; and with 8, still more. The stars that gradually made their appearance, therefore, were probably scattered over the space between the 48th and 96th order of distances.

In the 7 feet reflector, with the gaging powers 9 and 10, I saw a great number of stars; with 11 and 12, a greater number of stars and resolvable whitishness were seen; with 13 and 14, the number of visible stars was increased, and was so again with 15; with 16 and 17 in addition to the visible stars, there were many too faint to be distinctly perceived. These gages therefore extend the space over which the additional stars were scattered from the 96th to the 204th order of distances.

With a 10 feet reflector, reduced to a gaging power of 18, I saw a great number of stars: they were of very different magnitudes, and many whitish appearances were so faint that their consisting of stars remained doubtful. The power 19, which next I used, verified the reality of several suspected stars, and increased the lustre of the former ones. With 20, 22, and 25, the same progressive verifications of suspected

stars took place, and those which had been verified by the preceding powers, received subsequent additional illumination. With the whole space-penetrating power of the instrument, which is 28.67, the extremely faint stars in the field of view acquired more light, and many still fainter suspected whitish points could not be verified for want of a still higher gaging power. The stars which filled the field of view were of every various order of telescopic magnitudes, and, as appears by these observations, were probably scattered over a space extending from the 204th to the 344th order of distances.

As the power of the 10 feet reflector could not reach farther into space, I shall have recourse to some of my numerous observations made with the 20 feet telescope. In addition to 863 gages already published,* above 400 more have been taken in various parts of the heavens, but with regard to these gages, which on a supposition of an equality of scattering were looked upon as gages of distances, I have now to remark that, although a greater number of stars in the field of view is generally an indication of their greater distance from us, these gages, in fact, relate more immediately to the scattering of the stars, of which they give us a valuable information, such as will prove the different richness of the various regions of the heavens.

July 30, 1785. Right ascension $19^h\ 4'$. Polar distance $87°\ 5'$. The milky way is extremely rich in stars that are too small for the gage.

Dec. 7, 1785. Right ascension $5^h\ 33'$. Polar distance $66°\ 6'$.

* See Phil. Trans. for 1785, p. 221.

There are about 66 stars in the field of view, and many more so extremely small as not to admit of being gaged.

Sept. 20, 1786. Right ascension $20^h\ 40'$. Polar distance $54^\circ\ 36'$. There are about 80 stars in a quadrant, or 320 in the field of view, besides many more too small to be distinctly seen.

Oct. 14, 1787. Right ascension $21^h\ 57'$. Polar distance from $35^\circ\ 18'$ to $38^\circ\ 50'$. In this part of the heavens the large stars seem to be of the 9th and 10th magnitude. The small ones are gradually less till they escape the eye, so that appearances here favour the idea of a succeeding, more distant clustering part of the milky way.

Sept. 18, 1784. Right ascension $20^h\ 8'$. Polar distance from $70^\circ\ 9'$ to $72^\circ\ 49'$. The end of the stratum of the stars of the milky way cannot be seen.

By these observations it appears that the utmost stretch of the space-penetrating power of the 20 feet telescope could not fathom the Profundity of the milky way, and that the stars which were beyond its reach must have been farther from us than the 900dth order of distances.

I am far from limiting the milky way to the extent deduced from these observations; but as even the distance which has been stated may appear doubtful, I must repeat the argument which has been used with stars visible to the eye, but which now is greatly supported by telescopic vision. If the stars of the 5th, 6th, and 7th magnitudes cannot be supposed to be gradually of a smaller physical size and brightness than those of the 1st, 2d, and 3d, how much less can a supposition be admitted that would require that the stars, which by a long

series of gaging powers have been proved to make their gradual telescopic appearance from the 12th to the 900dth order of distances, should also be gradually of a different construction, with regard to physical size and brightness, from those which we see with the naked eye.

From the great diameter of the mirror of the 40 feet telescope we have reason to believe, that a review of the milky way with this instrument would carry the extent of this brilliant arrangement of stars as far into space as its penetrating power can reach, which would be to the 2300dth order of distances, and that it would then probably leave us again in the same uncertainty as the 20 feet telescope. When I made some sweeps of the heavens with the 40 feet telescope with a magnifying power of 370, I found it necessary to reduce the intended breadth of the sweep from one degree to 30 minutes, and the great length of time this would have taken up to examine only the ecliptic, to which I had directed the telescope, soon proved that by continuing to use this instrument for sweeping, I should have been obliged to neglect the necessary observations of the 20 feet telescope.

The following observations are extracted from my sweeps to support a few general remarks relating to the construction of the milky way.

Dec. 7, 1785. Right ascension $4^h\ 39'$. Polar distance from $64^\circ\ 0'$ to $66^\circ\ 12'$. The straggling stars of the milky way seem now to come on gradually; most of them are small. Right ascension $4^\circ\ 43'$. They begin now to be intermixed with some large ones.

This observation proves that the telescopic breadth of the milky way, considerably exceeds the extent which in our

maps is assigned to it. In this situation it began to appear at 6 or 7 degrees from where it might have been expected to enter the telescope.

Aug. 21, 1791. Right ascension 18^h 59′. Polar distance from 84° 15′ to 86° 17′. The milky way comes on very suddenly, and is amazingly crowded with very small stars intermixed with many of several sizes.

By our maps this place is already within the limits of the milky way.

Jan. 1, 1786. Right ascension 5^h 17′. Polar distance from 89° 28′ to 91° 47′. Most of the stars are larger than usual, but the whole breadth of the sweep contains a great mixture of all sizes.

From the brightness of the stars we may conclude that the constellation of Orion to which the observation belongs, is one of those that are nearest to our own situation.

Dec. 27, 1786. Right ascension 6^h 49′. Polar distance from 87° 37′ to 89° 55′. From the appearance of the heavens in this neighbourhood, there is reason to suppose that there is a break or vacancy among the stars between our situation and the more remote parts of the milky way.

The place to which this observation refers is in the breast of Monoceros.

Oct. 14. 1787. Right ascension 22^h 14′. Polar distance from 35° 18′ to 38° 50′. It is very evident that in this part of the heavens there is some distance between us and the milky way, which is not equally scattered over with stars.

The situation of the place pointed out by the observation is near the crown of Cepheus.

Sept. 15, 1792. Right ascension 19^h 46′. Polar distance

52° 29'. There are 153 stars in a quadrant, or 612 in the field of view, and the whole breadth of the sweep, which is 2° 2', is equally rich.

The gage was taken in the preceding branch of the milky way, in the neck of the Swan.

Aug. 22, 1792. Right ascension 19^h 35'. Polar distance 75° 5'. The field of view is extremely crowded, but the stars are too small and too numerous to be counted; there cannot be less than 100, or probably 150 in a quadrant. From some careful trials I suppose there were 150; this would give 600 stars in the field of view. The stars continue to be equally crowded throughout the whole breadth of the sweep, which was 2° 35' till right ascension 19^h 54', when there still were 440 in the field.

This gage was taken in the following branch of the milky way, in the wing of Aquila.

Sept. 13, 1784. Right ascension 20^h 48'. Polar distance from 54° 15' to 57° 1'. This branch of the milky way is less rich than the preceding one.

The same sweep passed through both the branches, and the observation relates to a place in the following wing of the Swan.

Oct. 19, 1788. Right ascension 21^h 13'. Polar distance from 43° 35' to 46° 13'. The milky way is very rich, but the stars are very unequally scattered.

This observation belongs to the tail of the Swan.

Nov. 26, 1788. Right ascension 23^h 40'. Polar distance 29° 13'. The milky way is very rich, but the stars are very unequally scattered. The stars are clustering. Right ascension 0^h 14'. Polar distance 29° 51'. Clustering small stars

considerably rich. Right ascension $0^h\ 27'$. Polar distance $30°\ 5'$. Clustering small stars.

These observations belong to a place preceding the back of Cassiopea's chair.

Dec. 27, 1786. Right ascension $6^h\ 36'$. Polar distance $88°\ 5'$. Clustering stars of the milky way, almost close enough, and so far separated from the rest of the stars as to be called a cluster, but still evidently joining to them.

This observation of the clustering of the stars of the milky way relates to a place that precedes the breast of Monoceros.

X. *Concluding Remarks.*

What has been said of the extent and condition of the milky way in several of my papers on the construction of the heavens, with the addition of the observations contained in this attempt to give a more correct idea of its profundity in space, will nearly contain all the general knowledge we can ever have of this magnificent collection of stars. To enter upon the subject of the contents of the heavens in the two comparatively vacant spaces on each side adjoining the milky way, the situation of globular clusters of planetary nebulæ, and of far extended nebulosities, would greatly exceed the compass of this Paper; I shall therefore only add one remarkable conclusion that may be drawn from the experiments which have been made with the gaging powers.

In fig. 2, Plate XV. let a circle, drawn with the radius of the 12th order of distances, represent a sphere containing every star that can be seen by the naked eye; then, if the breadth of the milky way were only 5 degrees, and if its profundity did not exceed the 900dth order of distances, the

Fig: 1.

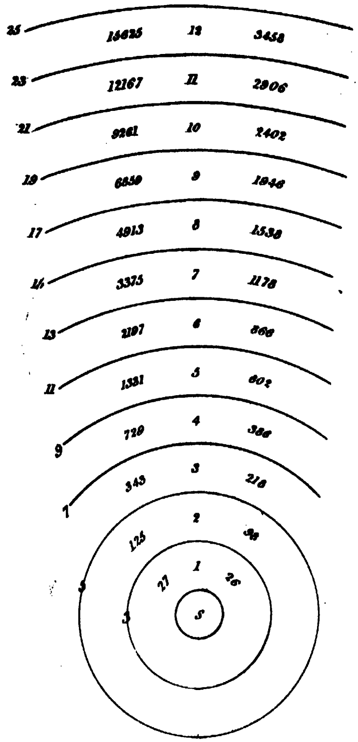
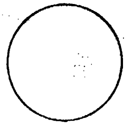


Fig: 2.



J. Basire sc.

two parallel lines in the figure, representing the breadth of the milky way, will, on each side of the centre of the inclosed circle, extend to more than the 39th order of distances.

From this it follows, that not only our sun, but all the stars we can see with the eye, are deeply immersed in the milky way, and form a component part of it.

WILLIAM HERSCHEL.

Slough, near Windsor,
May 10, 1817.

XXV. *Some account of the nests of the Java swallow, and of the glands that secrete the mucus of which they are composed. By Sir* EVERARD HOME, *Bart. V. P. R. S.*

Read June 26, 1817.

THE nests of a particular species of swallow which is principally met with in the island of Java, have from time immemorial formed an article of trade between that island and China, where they are purchased at a high price by that voluptuous people, it being believed, that the materials of which the nests are composed, are possessed of an aphrodisiac virtue in an eminent degree. They have been occasionally brought into this country, and are preserved in collections of natural history, as curiosities. In what manner the bird procures the materials out of which the nest is made, has till now remained unknown; a thousand conjectures have, however, been made upon this subject. It has been supposed by some, that it is a gluten collected from the mollusca picked upon the surface of the sea. By others, a substance extracted from certain fuci found on the sea shore. By others again, a portion of the food in a half digested state regurgitated to be employed for this particular purpose. Sir STAMFORD RAFFLES, who has just returned from Java, where he resided five years, as lieutenant governor, has brought over a number of these nests, and has been kind enough to offer me some of them, for the purpose of investigating the nature of

the materials of which they are composed, and gives it decidedly as his own opinion, that, whatever it is, it is brought up from the stomach, and requires at times so great an effort, as to bring up blood, the stain of which is seen on the nest. This account of Sir Stamford Raffles, in the correctness of whose observation I have the greatest confidence, led me to investigate this subject, and to ascertain by examination whether this particular swallow has any glands that are peculiar to its œsophagus, or stomach, enabling it to secrete a mucus similar in its nature to the substance of which the nest is composed. I at the same time requested my friend, Professor Brande, to analyze one of the nests, and to inform me of its composition. In examining the gastric glands of the Java swallow, even with the assistance of a common magnifying glass, I saw an obvious difference between the appearance of the orifices by which the secretion is poured into the gizzard, and of those of other birds, but, as I had never examined those glands in the common swallow which migrates to this country, it became necessary, before I proceeded farther in the enquiry, to ascertain whether in all the swallow tribe there were similar structures. In the present season this opportunity has been afforded me, and I find that in the common swallow, both male and female, the orifices of the gastric glands differ in nothing from those of birds in general, but that the peculiar structure which I am about to describe is confined to the Java swallow. This bird, Sir Stamford Raffles informs me, does not migrate, but remains all the year an inhabitant of the caverns in that island. Some of the most extensive caves in which they reside, are forty miles from either sea. Those swallows that build their nests near the

sea, are observed to fly inland towards extensive swamps where gnats and other insects are in great abundance. Those that build in inland caves, are observed to quit the caves in the morning, and generally return in swarms darkening the air, towards the close of the day; they are, however, going in and out the whole of the day. This bird is double the size of our common swallow. There are two separate nests, one for the male to lie and rest in, which is oblong and narrow, adapted to his form, the other wide and deeper, to receive the female and the eggs.

As Mr. BAUER has been kind enough to make drawings of the gastric glands in the blackbird, the common swallow, and Java swallow, in which the parts are so much magnified, that the difference in their structure is obvious to the most superficial observer, it is not necessary in this place to enter much into detail respecting them: I shall only observe, that from what is represented in the drawings, it is evident that the gastric glands in the swallow tribe, both those that migrate and those that remain during the whole year in Java, do not afford the same supply of gastric liquor as in other birds, since they have a smaller receptacle belonging to the gland into which the secreted liquor is to be received. This circumstance confirms the observations that I made, upon a former occasion, respecting the gastric glands of the casuary of Java and of the ostrich, that these glands are largest in those birds that inhabit countries that afford a small supply of nourishment. The swallow of Java, as well as the casuary of that island, lives in perpetual plenty, and the swallow that migrates, although it travels from the equator to the pole, only remains in cold countries during the summer season,

while the sun is fertilizing them, and therefore has probably an equally abundant supply of its natural food in the regions of the north, as at the equator.

The only difference between the glands of the migrating swallow, and those of the blackbird, is the smallness of the reservoir, the surface of the gullet upon which the external openings of the glands are seen is exactly the same, there is not in the one or the other any apparatus for secreting mucus which is not common to birds in general.

In the Java swallow we have, on the other hand, a structure of a particular nature; there is a membranous tube surrounding the duct of each of the gastric glands, which, after projecting into the gullet for a little way, splits into separate portions like the petals of a flower: for what purpose so extraordinary an apparatus could be provided, would probably have puzzled the weak intellects of human beings, and many wild theories might have been formed respecting it, had not the animal matter of which the bird's nest is composed, and the accurate observation of Sir STAMFORD RAFFLES, who had no doubt that the materials of the nest were produced from the gullet, led to the discovery of its use.

That the mucus of which the nest is composed, is secreted from the surface of these membranous tubes, there is no more doubt than that the gastric juice is secreted from the glands whose ducts these tubes surround; and this confirms an opinion which I have adopted for many years, that membranes on which no glandular structure could be seen, were capable of secreting mucus; and now that we find those membranes, where their surfaces are so much magnified, exhibit no glandular structure, we may, without the chance of

more accurate observers refuting us, be satisfied that no such structure exists.

'There are, perhaps, no more curious provisions given to animals by their Creator, than those which are to be employed for the preservation of their young, while it yet remains in the egg; but as many of these belong to the organs of generation themselves, or arise from secretions produced by glands immediately connected with them, they pass unnoticed, the mind being lost in the contemplation of so wonderful a contrivance as generation itself.

The present provision for forming a nest out of its own secretions, in an animal of so high an order as the class Aves, strikes us with astonishment, since birds in all other countries find substances of some kind or other out of which they form their nests, and makes it evident that this particular bird, at the time of its first creation, was intended to be the inhabitant of the caverns of Java, in which nothing is to be met with out of which a nest could be constructed, as the camel is adapted to the sandy deserts of which it is the natural inhabitant, both by the provision in its stomach for carrying a store of water, and the form of its hoof, which cannot, like that of other animals, be injured by walking in sand.

The swallows of Java that reside upon the coast, never exhaust their secretions in forming their nests when they find other materials fitted for that purpose.

The nearest approach to a provision of this kind, is in the insect tribe, the bee secreting wax out of which it forms its comb, both for the nest of its young, and a reservoir to contain supplies of nourishment.

The nest described in the preceding Paper, appears to con-

sist of a substance having properties intermediate between gelatine and albumen. It resists for a considerable time the action of warm water, but after some hours enlarges and softens; upon drying, it again resumes its former appearance and properties, becoming somewhat more brittle than before, probably, in consequence of having lost a very small portion of gelatine, which delicate tests discover in the water.

In the diluted acids, this substance dissolves with more ease than coagulated albumen; in the concentrated acids, its properties are nearly the same as those of coagulated white of egg.

With the caustic and subcarbonated fixed alcalis, it forms saponaceous compounds, which are decomposed by the acids with the same appearances as other albuminous soaps. It readily dissolves in liquid ammonia, and in the solution of subcarbonate of ammonia, circumstances in which it differs from albumen. When submitted to destructive distillation, a relatively small portion of ammonia is formed, and the remaining coal is easy of incineration, circumstances which likewise lead to point out a distinction between this substance and albumen.

Explanation of Plate XVI.

Fig. 1. The gizzard and part of the gullet of the Java swallow slit up and laid open, magnified twice in diameter, or 4 times in superficies.

Fig. 2. A small portion of the lowermost part of the gullet, magnified 15 times in diameter, or 225 times in superficies.

Fig. 3, 4, and 5. Side view of the glands of different sizes.

Fig. 6. Perpendicular section of Fig. 5.

Fig. 7. A top view of one of those glands; the figures 3, 4, 5, 6, and 7, are magnified 50 times in diameter, or 2500 in superficies.

Fig. 8. A small portion of the lowermost part of the gullet of the common swallow.

Fig. 9. A small portion of the lowermost part of the gullet of the black bird, both figures are magnified 15 times in diameter, or 225 times in superficies.

Fig. 1.

Fig. 2.

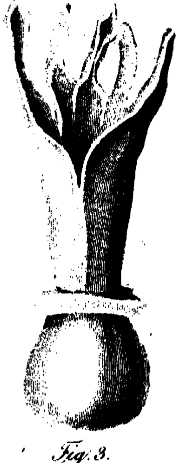


Fig. 3.

Fig. 4.

Fig. 5.

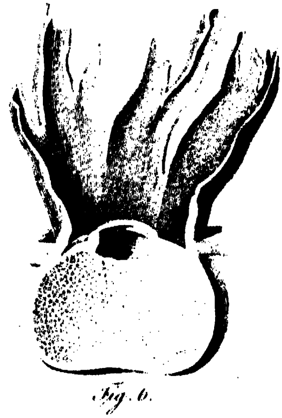


Fig. 6.

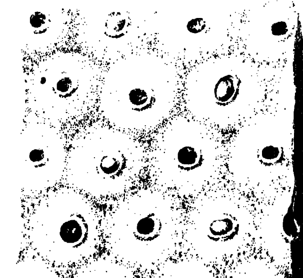


Fig. 8.

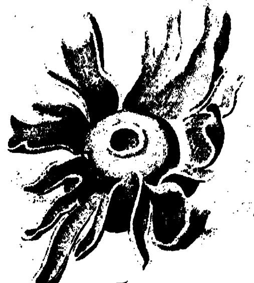


Fig. 7.

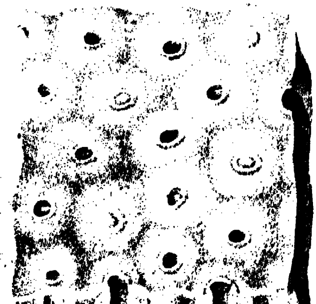


Fig. 9.

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XXVI. *Observations on the Hirudo complanata, and Hirudo stagnalis, now formed into a distinct genus under the name,* Glossopora. *By Dr.* Johnson, *of Bristol. Communicated by Sir* Everard Home, *Bart. V. P. R. S.*

Read June 26, 1817.

I beg leave to lay before the Society, a few remarks on the *Hirudo complanata* and *Hirudo stagnalis;* animals that have been hitherto, but injudiciously, retained in the genus Hirudo.

From the circumstance, in which they differ more particularly from the leech, they are now formed into a distinct genus, under the term of *Glossopora.*

These animals resemble the leech,

a. In the body being furnished with a series of rings.

b. In locomotion being effected by the alternate attachment of the head and tail.

c. In the division of one general stomach into several lateral cells or partitions.

These animals differ from the leech,

a. In the mouth being furnished with a projectile tubular tongue.

b. In the body being nearly flattened, and pyriform.

c. In having an abdominal pouch, or cavity, for the reception of their young.

The genus which they now form, takes the name (from γλῶσσα, a tongue, and πόρος, an aperture) of Glossopora.

CHARACTER GENERIS.

Corpus subovatum, depressum, caput acuminatum, lingua tubulata resiliens, os caudamque alterne affigendo progrediens.

G. tuberculata: *Glossopora dilatata, supra cinerea linea duplici tuberculata, subtus grisea, atomis nigris innumeris.*

LINNÆUS.	*Syst. Nat.* XII. 2. p. 1079. n. 6.
———	*Faun Suec.* 2082, Hirudo depressa ovato-oblonga interaneis fuscis pinnatis pellucentibus.
HILL.	*Hist. Anim.* p. 16. Hirudo lateribus attenuatis.
BERGMANN.	*Act. Stockh.* 1757, Tab. 6. Fig. 12, 14. Hirudo sexoculata.
MULLER.	*Hist. Verm.* 2. n. 157, p. 47. Hirudo complanata.

G. punctata: *Glossopora gracilis, cinereo-viridis, punctis plurimis subnigris.*

LINNÆUS.	*Syst. Nat.* XII. 2. p. 1079. n. 5.
———	*Faun. Suec.* 2081. Hirudo (stagnalis) depressa nigra, abdomine subcinereo.
BERGMANN.	*Act. Stockh.* 1757, n. 4. Tab. 6. Fig. 9—11. Hirudo binoculata.
MULLER.	*Hist. Verm.* 2. n. 171. p. 41. Hirudo bioculata.

I am of opinion, that the *Hirudo circulans* described by Mr. SOWERBY,* and the *Hirudo crenata* by the Rev. WILLIAM KIRBY,† belong to this genus; but having had no opportunity of seeing either, I cannot determine whether or not they possess the tubular tongue.

* British Miscellany, Tab. 76.

† Linn. Trans. vol. ii. p. 316.

The *Hirudo Hyalina* which, MULLER observes, has a flattened body, and carries its young in a pouch, and the *Hirudo tessulata* of the same author, will, I think, also be found upon examination, to belong to this new genus.

The tubular tongue very seldom falls within our view; hence our surprise is the less, that it should so long have escaped notice. At the time I first observed it, I was unacquainted with any author, who had mentioned it. On my referring, however, to BERGMANN's account of the *Hirudo sexoculata,* (now *G. tuberculata*) I find it there noticed, not as a tongue, but as a slender body, of a whitish colour, occasionally projected from the mouth; of the use of which he confesses himself to be ignorant. I give his words, "*Utur munnen har jag atskille gånger sett uträckas en blek, smal lem, hvars nytta är mig obekant.*"* MULLER mentions that he never witnessed (although he frequently looked for it) the body which BERGMANN saw the *Hirudo sexoculata* thrust from its mouth: but he once observed the *Hirudo vulgaris* protrude a similar organ, when, he says, this assertion of BERGMANN came across his mind. On his observing it, however, more narrowly, it proved to be a small aquatic worm that the animal had swallowed and afterwards rejected. This readily accounts, he adds, for the mistake into which BERGMANN has fallen.

In stepping forward to support BERGMANN, I am only doing an act of justice to the merits of an accurate and intelligent observer.

Having had the *G. tuberculata* and *G. punctata* under my

* Stockholm Transactions, for 1757, p. 313.

daily notice for a period of at least six months, I may, I presume, speak with some decision on this point. It may seem perhaps unnecessary to add, that I possess an elegant preparation, showing this tongue (protruded from the mouth) filled with mercury. It is of a cartilaginous structure, and admits of great flexibility. It is in length about one-eighth of an inch, and is seen, delineated (magnified) in Fig. 5. (Pl. XVII.)

The *G. tuberculata*, is about half an inch in length, but when fully extended, one inch. It is commonly found in rivulets, attached to pieces of wood, stones, &c. A delineation is given of its natural size, in a front and back view, in Fig. 1, 2, 3, 4. It possesses great transparency, and has a fine glossy vitreous appearance. It is convex above, flattened beneath, and somewhat resembles a compressed pear, the tail being very broad, and the head tapering towards the extremity, in which may be seen six eyes, (Fig. 9. *b*) disposed in two longitudinal rows. The sides or margin of the body are serrated. The back is usually of a brown colour, with lighter or darker patches, ornamented in the middle with a double longitudinal row of white tubercles. (Fig. 9. *c*.) These tubercles are connected together by two black longitudinal lines, and are seldom apparent, unless the animal is at rest. The belly is generally of one uniform colour, chiefly grey, with a slight double black line running longitudinally in the centre.

When this animal is in motion, (to which it is much averse, seldom quitting the spot on which it may be affixed,) it is observed to throw forward its head to the greatest point of extension, and then attach itself by means of the sucker ter-

minating that extremity. Thus securing its hold, it draws up the tail, when the back describes an arch, as in Fig. 6. The tail is then fixed by a similar sucker, and the head is again extended. Now and then it supports itself by the tail, the head waving to and fro, and occasionally buries its head under the abdomen, somewhat after the manner of an *oniscus*. This is seen in Fig. 7.

The *G. punctata* is much smaller than the above, and exceedingly delicate in its structure. On the head, we notice two eyes, placed transversely (Fig. 13. *b*). It is delineated of its natural size, in Fig. 11, 12. Both the back and belly is of a dusky grey, profusely sprinkled with minute black specks.

The stomach of the *G. tuberculata*, like that of the leech, is divided into several cells or partitions (Fig. 10. *d. e.*) with their extreme points verging towards the tail. The two last of these cells (*f*) are much longer than the rest, and terminate in two blind sacs. Between them, we notice the intestine, a tortuous tube extending to the anus, (Fig. 9. *e.*) a foramen above the rim of the circular sucker, or what constitutes the tail.

These animals, it has been asserted, when cut, or divided, are capable of reproduction, but this seems to rest on no just foundation.

Their food principally consists of the *water helices*, and here we see the great use of the projectile tubular tongue. The animal, from its tapering so much towards the head, is enabled to penetrate some considerable way into the hollow of the shell, and from the flexibility of its tongue, can follow its victim to the innermost recess of its habitation. A passage

from MULLER, upon this subject, may not be devoid of interest. "Spectaculum singulare præbuit hujus (*G. punctata*, olim *H. bioculata*) cum limace *Planorbis* conflictus: limacem ore prehendere molitur hirudo; ille se quam citissime cum strepitu ex aeris et aquæ subitanea pressione orto testa condit. Hirudo oram aperturæ tentare pergit, at Limax insidias sentiens, seque in domuncula hunc contra hostem minus tutum credens, animum capit, egreditur et festinanter ad summum vasculi marginem prorependo ex aqua aufugit. Miratu dignus *Limacis* instinctus salutem quærendi fuga in elementum *Hirudini* contrarium;

> Omnibus ignotæ mortis timor, omnibus hostem
> Præsidiumque datum sentire, et noscere teli
> Vimque modumque sui.

Paucas tamen post horas, jubente natura, in aquam rursus descendere coactus, novo sese periculo obtulit, eique demum succubuit."

The *G. tuberculata*, and *G. punctata*, are oviparous; the former producing about 50, the latter about 20 at a birth. The same appearance is observed in these animals as in the *H. vulgaris*, when they deposit their ova, that is, a contraction of the body both above and below the abdominal foramen (Fig. 8.). There is, however, this difference; the *H. vulgaris* deposits its ova *in a capsule, formed exterior to the body*; whilst in these animals, the ova are simply excluded, held together by some gelatinous matter. From six to twelve ova are deposited at a time. When the whole of the ova are excluded, they are received into the abdominal pouch of the parent, where they constantly remain, until their contents are fully evolved. If the ova are removed, and kept in a vessel by

themselves, they do not prove productive; hence there seems to be a necessity for this parental solicitude. This pouch, or cavity, is always conspicuous in the *G. punctata*, but in the *G. tuberculata*, only at the time of its producing young. When the young are excluded from the ova, they remain attached to this cavity by the tail, enjoying a free extent of motion with the rest of the body. In this position they are represented in Fig. 7. They frequently leave this pouch, but soon return, and again affix themselves. Shortly after birth, their *interanea* are filled with a cream coloured fluid, which, under the microscope, presents a most interesting and beautiful appearance. Whilst speaking of the stomach of the parent animal, I forgot to observe, that the *interanea* are only visible when food has been recently taken. I mention this circumstance, that I may not be supposed to labour under a mistake, in the view I have given of this organ. MULLER himself indeed confesses, that he was a whole month (although assisted by the microscope) before he discovered it, yet he with much candour adds, that he afterwards very frequently saw it, even with the naked eye.

JAMES RAWLINS JOHNSON.

London, June 18, 1817.

EXPLANATION OF PLATE XVII.

Fig. 1, 2. The *G. tuberculata*, natural size, front view.

Fig. 3, 4. Ditto, back view.

Fig. 5. The tubular tongue (magnified): that portion from *a* to *b* is usually protruded from the mouth, the letter

c refers to the root or the expanded part of the tongue, or what, more properly speaking, constitutes the œsophagus.

Fig. 6. Shows the arched back the *G. tuberculata* presents, when in motion.

Fig. 7. Shows the abdominal pouch of this animal with the young affixed to it by their tail, enjoying free motion with the rest of the body.

Fig. 8. Its appearance when about to deposit its ova; *a*, the mouth; *b*, the contracted portion containing the ova to be then excluded; *c*, the remaining portion of the ova left in the abdominal cavity; *d*, the tail.

Fig. 9. The *G. tuberculata* magnified, showing, *a*, the tubular tongue; *b*, the eyes; *c*, the double longitudinal row of white tubercles; *d*, the serrated margin; *e*, the anus.

Fig. 10. The back part of the same animal, showing, *a*, the tubular tongue projected from the mouth; *b*, the œsophagus; *c*, the abdominal foramen; *d*, the alimentary canal or stomach; *e*, the lateral cells of the stomach; *f*, the two last long cells; *g*, the intestine; *h*, the sucker, or tail.

Fig. 11. *G. punctata*, natural size, front view.

Fig. 12. Ditto, back view.

Fig. 13. The same animal, magnified, showing, *a*, the projectile tubular tongue; *b*, the eyes; *c*, the abdominal pouch; *d*, the tail; *e*, the serrated margin.

XXVII. *Observations on the gastric glands of the human stomach, and the contraction which takes place in that viscus.* *By Sir* Everard Home, *Bart. V. P. R. S.*

Read June 26, 1817.

In the year 1807, I laid before the Royal Society some observations on the human stomach, and I am now enabled, through the assistance of Mr. Bauer, to exhibit magnified views of the internal membrane of that viscus, in which the different structures composing its surface are distinctly shown.

The magnified drawings of the gastric glands of the Java swallow, so lately exhibited, must still rest upon the minds of those members who saw them; they are of so instructive a nature, that I was led to request Mr. Bauer would make similar representations of the glands of the lower part of the human œsophagus, and of the surface of the internal membrane of the stomach and duodenum.

The stomach employed for this purpose was under the most favourable circumstances, as the patient had died of an apoplexy, having no other bodily complaint.

The glands situated in the lining of the lower part of the œsophagus, which in my former Paper were called œsophageal glands, when examined in the microscope, have the appearance of infundibular cells, whose depth does not exceed the thickness of the membrane. This structure, however different from that of the gastric glands of birds, is a nearer

approach to it, than is to be met with on any part of the internal surface of the stomach or duodenum; it also resembles them in the secretion it produces coagulating milk, and none of the inspissated juices met with in these cavities affect milk in the same way. From these facts there can no longer be any doubt entertained, that the gastric glands have the same situation respecting the cavity of the stomach as in birds.

In my former investigation, the analogy of the bird would have led me to the same conclusion, had not the gastric glands of the beaver, which are more distinct than in any other quadruped, been a stumbling block in my way; but now the situation of these glands in the beaver and woombat, must be considered as an exception to the general rule, the necessary complexity of their structure making them too large to admit of their being conveniently placed, as is usual, in the œsophagus.

The structure upon the upper arch of the stomach, which, when magnified by a common lens, had the appearance of glands, is shown by Mr. BAUER to be made up of cells in the form of a honeycomb, the sides of which are not formed by doublings of the membrane, for no stretching of the cells alters the form of their orifices, but are regular partitions constructed between the cells. This honeycomb structure consists of cells of the greatest depth in this particular situation, but it is met with over the whole surface of the cardiac portion of the stomach, only the appearance is so faint as to require a great magnifying power to render it visible. In the pyloric portion the cells, in general, have the same appearance, but there are small clusters, the sides of which

rise above the surface, giving the appearance of foliated membranes. In the duodenum this takes place in a greater degree, and the loose edges of these membranes, when entangled in the mucus that covers them, puts on an appearance of rounded glandular bodies, but these admitted of being expanded so as to explain the deception.

The description which I have given of the internal membrane of the stomach, proves how nearly my late ingenious friend, Dr. GEORGE FORDYCE, who examined its surface with very inferior means to those employed upon the present occasion, had approached the truth, when he declared it to be composed of cellular membrane.

I have shown upon more occasions than one, that the gastric glands are both largest and most numerous in those animals destined to inhabit the least fertile regions of the earth, and are smallest as well as fewest where the supply of food is most abundant, to prevent the body being injured by the effects of over feeding. If this arrangement was necessary in animals, it became still more so in man, whose means of procuring and preparing food for himself so much exceed those of all other animals, and who is, contrary to his reason, too readily disposed to carry the indulgencies of the table to excess. In him the gastric glands, as it was natural to expect, are so small, as to require the aid of Mr. BAUER's microscope to prove that they belong to the same series of structures as the gastric glands of the ostrich, which admit of being minutely examined by the naked eye.

Much is still wanting to enable us to understand the process of digestion: it is, however, no small step in this investigation, that a more correct knowledge of the structure of the

cavities in which it takes place, has been acquired; from which we learn that there are three different kinds of organization employed, in adding to the food three different ingredients which are requisite for its conversion into a material that can be assimilated with living animal matter, and be employed in carrying on the functions of life, also supplying the waste which is constantly taking place. The most important of these is evidently the gastric glands, next in order may be considered the honeycomb structure, and least so, although by no means unnecessary, the foliated membranes, which we know, from what takes place in the Java swallow, form the mucus that is mixed with the other ingredients.

That the stomach is occasionally met with after death divided into two portions by a muscular contraction, I have shown upon a former occasion, and I have there given it as my opinion, that this takes place while the process of digestion is going on. This opinion may, in the minds of many physiologists, require some stronger proofs than I have been able to give, but this, like many other muscular contractions which take place during life, is so often removed in the very act of dying, as rarely (and in some of them never) to be seen after death. We are therefore indebted to the effect of disease sometimes rendering them permanent, for any knowledge we have of their existence.

In this way, strictures in the œsophagus, just where the fauces terminate, and the œsophagus begins, being of frequent occurrence, and upon examination of the parts after death, found to have taken place unconnected with any disease in the surrounding parts, teaches us, that this part has an involuntary contraction when any irritating matter is applied, and

thus forms a guard to prevent substances that would prove hurtful to the stomach from being swallowed.

Strictures in the urethra immediately behind the cavity of the bulb, being met with under the same circumstances after the use of irritating injections into that canal, and various other causes of irritation, is the only evidence we have of this part having a power of involuntary contraction, which in the act of the coitus is employed to prevent any part of the semen from being forced backwards into the bladder.

Through the kindness of Mr. CARPUE, I am now enabled to produce a specimen of permanent contraction in the stomach, and if I had not observed such a contraction before, this specimen would have led, as in the other cases just mentioned, to the discovery of the stomach in some of its natural actions, having this kind of contraction take place in it. It was met with in the body of a woman, and was probably the cause of her death, as no other appearance of disease was met with: the body was exceedingly emaciated, but there was no opportunity of acquiring any information of the symptoms under which she laboured while alive.

As in this instance the stomach could be distended freely without any risk of the contraction giving way, the line of partition between the cardiac and pyloric portions is exactly defined, and shown in the drawing not to be the casual contraction of a few of the transverse muscular fibres, which might have happened equally to any of the others, but the contraction of a part that had always been liable to it, and which was to answer some purpose in the performance of the functions of that viscus.

The importance of this fact in studying the physiology of

the stomach, is the only apology I shall make for having pressed it so much upon the attention of the Society. Its use in the pathology of that viscus, although perhaps of still more importance to the cause of suffering humanity, this is not the proper place to consider.

EXPLANATION OF PLATES XVIII, XIX, XX.

PLATE XVIII.

Fig. 1. $\frac{6}{100}$ parts of a square inch of the lower part of the œsophagus, magnified 15 times in diameter, or 225 times in superficies.

Fig. 2. $\frac{2}{100}$ parts of a square inch of the cardiac portion, magnified 15 times in diameter, or 225 times in superficies.

Fig. 3. $\frac{2}{400}$ parts of a square inch of the same cardiac portion, magnified 30 times in diameter, or 900 times in superficies.

PLATE XIX.

Fig. 1. $\frac{2}{100}$ parts of a square inch of the pyloric portion, magnified 15 times in diameter, or 225 times in superficies.

Fig. 2. $\frac{2}{400}$ parts of a square inch of the same pyloric portion, magnified 30 times in diameter, or 900 times in superficies.

Fig. 3. $\frac{6}{100}$ parts of a square inch of the duodenum, immediately joining the pylorus, magnified 15 times in diameter, or 225 times in superficies.

PLATE XX.

The human stomach, in a distended state, to show a permanent contraction, which had taken place in consequence of disease, separating the cardiac from the pyloric portions.

XXVIII. *On the parallax of the fixed stars.* *By* John Pond, *Esq. Astronomer Royal.*

Read June 26, 1817.

In a former Paper on the subject of parallax, I mentioned my intention of prosecuting this investigation by means of fixed telescopes on a new construction, and which I conceived were better adapted to this purpose, than any other instrument hitherto employed. One of these was directed to α Aquilæ, the other to α Cygni. Both instruments have answered the intended purpose. The observations of α Aquilæ were interrupted by some alterations which I found it necessary to make in the construction of the instrument; but those of α Cygni have been continued without intermission from the month of January to the present time.

Strictly speaking, a much longer period would be required (even with a perfect instrument) to determine either the exact quantity of parallax, in case it could be discovered, or the limit which it does not exceed, should it prove to be too small to be susceptible of observation; since the uncertainty in the annual variation of each star, bears a considerable proportion to the quantity to be investigated.

Notwithstanding, however, this very small uncertainty which future observation alone can remove, it appears to me that the observations already obtained, afford a proof sufficiently satisfactory, that the discordances which formed the subject of my last communication, must have had some other cause than parallax.

Though the method I have adopted in this investigation is extremely simple, I am not aware that it has been ever employed before. I do not attempt to refer the position of the star whose parallax is to be ascertained to any point determined by a level or plumb line, but by means of a micrometer within the telescope I measure only its difference of declination from some other star which passes through the same field; the telescope itself being securely fixed on a stone pier. The star chosen for this purpose should differ as much as possible in right ascension from the star whose parallax is to be investigated, that the results may be affected by the sum of the parallaxes of both stars. It should likewise be sufficiently bright to be visible even when it passes the meridian at noon, otherwise the observations must necessarily be subject to a period of interruption at the very time they would be of the most importance. Should the difference of declination of the two stars exceed 8 or 10 minutes, it will be advisable to apply a double micrometer to the telescope; that is, a micrometer having two moveable wires, such as have been made for some years past by Mr. Troughton, and are, I believe, familiar to most astronomers of this country.*

The star I have selected is β Aurigæ, and the annexed observations from Jan. 21st, to May 16th, will show to what a very singular degree of precision results may be obtained.

* As Mr. Troughton's numerous avocations could not have permitted him to satisfy my impatience, which made me very anxious not to lose a season, the instrument was made by Mr. Dollond. It was executed in great haste (being completed in less than three weeks); every part of it, nevertheless, is finished with such care and precision, that I regard it as a most valuable acquisition to the Observatory, and worthy to hold a place in a collection of instruments, which I believe cannot be equalled in Europe.

Result of 54 *observations of* α *Cygni compared with* β *Aurigæ, from* 21*st January* 1817, *to the* 16*th of May.*

	α Cygni +	β Aurigæ =	Mean of each series of six Observations	Mean of each series of 18 Observations
Jan.	8″.197 +	9″.705 =	17″.902	
	8.295 +	9.673 =	17.968	8″.249 + 9″.757 = 18″.006
	8.255 +	9.892 =	18.147	
	7.913 +	10.058 =	17.971	
	8.225 +	9.966 =	18.191	8.133 + 9.861 = 17.994
	8.262 +	9.560 =	17.822	
	8.047 +	10.072 =	18.119	
May	7.553 +	10.536 =	18.089	7.604 + 10.473 = 18.077
	7.213 +	10.812 =	18.025	

As it so happens that δ Cygni and *b* Aurigæ pass through the field of the telescope, they have likewise been observed.

Observations of these stars cannot be made directly useful in investigating the parallax of α Cygni, but may be of some importance in determining other equations; they likewise serve to show the limits of error to which the instrument may be liable.

The above observations do not include the extreme positions of the star in which it arrives at its maximum and minimum of parallax. As it appeared desirable to extend the period of observation as much as possible, I requested permission to insert the continuation of them to the time the impression of my Paper was required to complete the volume. They are therefore continued to the 21st of September, at which

time the summer observations may be presumed to terminate. Beyond this period, the observations rather belong to the autumnal or neutral state of parallax.

On the result of the observations thus continued, I beg leave to add the following remarks.

If the total number of observations be divided into three periods according to the state of the combined parallax, the result will appear to be as follows.

Winter Observations.	Rev.	″		Rev.	″
25 α Cygni	24 +	8.173	their sum =	59 +	17.984
28 β Aurigæ	35 +	9.811			
Neutral	Rev.	″		Rev	″
26 α Cygni	24 +	7.920	their sum	59 +	17.964
29 β Aurigæ	35 +	10.044			
Summer	Rev.	″		Rev.	″
32 α Cygni	24 +	3.340	their sum	59 +	18.165
22 β Aurigæ	35 +	14.825			

The summer of this year has proved the most unfavourable for observation of any I recollect for several years. Many of the observations of β Aurigæ were extremely unsatisfactory. Notwithstanding this, I do not consider the small discordance of 0″.2 in the total distance of the two stars in a direction contrary to the effect of parallax as accidental, but trust that in time I shall discover the cause. For the present, however, I am willing to attribute it to error of observation. Now, even in this most unfavourable point of view, I think we may venture to infer, that (supposing the proportion of parallax of each star not to differ greatly from their degree of brightness) the mean place of either of them *is never deranged* by parallax *above one tenth of a second,* because such a deviation would produce, by its double effect on each star, a

total discordance of nearly $0'',3$, which I really think exceeds the limits of my uncertainty.*

Should the parallax of α Cygni be admitted to be insensible, it would follow from the observations with the mural circle, that it was equally so in α Lyræ and γ Draconis, as these stars have assiduously been observed during a period of 5 or 6 years, without any perceptible difference at opposite seasons.

Of α Aquilæ, I cannot speak with the same confidence, but as I propose to make my observations of this star the subject of a separate communication, I need not now anticipate it, and shall only observe, that in a star so far from the Zenith, it would be rather unsafe to ascribe any small discordance to parallax, except confirmed in brighter stars more advantageously situated for observation.

TABLE I. Contains all the observations made with the instrument corrected for the usual equations, β Aurigæ is observed on the northern wire, the other three stars on the southern wire.

TABLE II. Contains the results arranged in three series.

TABLE III. Contains in a similar manner the results of α Cygni and δ Cygni, but the early observations of δ Cygni are not to be relied on, the star being scarcely visible from its vicinity to the sun, the uncertainty I think does not exceed $0'',25$. It is evident that they both have the same parallax.

TABLES IV. and V. Give the coefficient of parallax, and require no explanation.

* This very small uncertainty may, I think, be somewhat diminished by the following consideration: the mean of all the observations in March, April, September, and October, should give the summer distance of these two stars under any theory either of parallax or annual motion. Now the mean of 50 observations in these months is $18''.15$. The exact coincidence of this quantity with that found by actual observation, indicates that the small discordance above noticed is not accidental, but in its progress is something similar to annual variation.

In the case of the Pole star, I find an annual variation that cannot be deduced from any comparison of distant observations.

TABLE I.

1817.	β Aurigæ Revolutions 35 +	α Cygni Revolutions 24 +	δ Cygni Revolutions 12 +	b Aurigæ Revolutions 12 +	1817.	β Aurigæ Revolutions 35 +	α Cygni Revolutions 24 +	δ Cygni Revolutions 12 +	b Aurigæ Revolutions 12 +
	″	″	″	″		″	″	″	″
Jan. 21	9.53	8,36	1,51	—	Mar. 22	9 71	8,00	3.39	10,71
22	—	—	—	—	23	—	8,50	—	—
23	9,65	—	—	—	24	—	—	—	—
24	—	—	—	—	25	10,25	—	—	11,15
25	—	—	—	—	26	—	8,13	—	—
26	—	—	—	—	27	9.73	—	—	—
27	—	—	—	—	28	—	—	—	—
28	—	—	—	—	29	—	8,55	3,65	—
29	—	—	—	—	30	—	—	—	—
30	9 84	8.44	—	—	31	9.72	8,07	3.54	—
31	9.87	8,18	1,85	—	Apr. 1	8,95	—	—	—
Feb. 1	8,79	—	—	—	2	9.27	8,59	4.53	—
2	—	—	—	—	3	9.14	8,53	4.07	—
3	—	—	—	—	4	9.70	—	—	—
4	—	8,25	1,15	—	5	9.57	8,33	3.24	—
5	—	—	—	—	6	—	—	—	—
6	9.53	8,19	—	—	·	9,48	—	—	—
7	—	7,76	—	—	8	9.52	—	—	—
8	9,81	—	—	—	9	—	7,84	—	—
9	—	—	—	—	10	:11,24	8,21	—	—
10	—	—	—	—	11	10,30	—	—	—
11	9.57	—	—	9.70	12	—	—	—	—
12	—	—	—	—	13	—	—	—	—
13	—	8,15	—	—	14	9.95	—	—	—
14	9.72	—	—	10,86	15	10,10	—	—	—
15	9,61	8,24	—	10,49	16	10,45	7.55	—	—
16	9.54	—	—	10,34	17	—	8,25	—	—
17	—	—	—	—	18	—	7,67	—	—
18	8,75	8,74	—	—	19	10,38	8,25	—	—
19	9.72	:7,27	—	—	20	9 80	8,01	—	—
20	—	—	—	—	21	10,71	—	—	—
21	10,26	8,51	—	—	22	8.95	—	—	—
22	9,85	—	—	10,24	23	10,24	8.55	—	—
23	—	8,33	—	—	24	—	—	—	—
24	10,04	7.80	2.83	—	25	—	—	—	—
25	10,02	8,36	2,55	—	26	—	8,57	—	—
26	—	8,28	—	10,94	27	—	—	—	—
27	9.54	—	—	10,67	28	—	—	—	—
28	9,68	8,31	—	10,61	29	—	—	—	—
Mar. 1	—	8,00	—	—	30	10,49	—	—	—
2	—	—	2,38	—	May 1	—	7.74	—	—
3	—	8,53	2,67	—	2	—	7.48	—	—
4	10,06	—	2,88	9,66	3	—	—	—	—
5	10,16	8.00	—	9,60	4	11,13	7,66	—	—
6	10,16	7.75	3.22	9.98	5	9,21	8,11	—	—
7	10,27	—	—	—	6	10,80	7.15	—	—
8	9,91	7.25	—	—	7	10,05	—	—	—
9	10,46	7,56	—	10,15	8	—	—	—	—
10	—	8,04	3,82	—	9	—	—	—	—
11	—	8,43	—	—	10	—	7.14	—	—
12	—	—	—	—	11	—	7.45	—	—
13	—	8,45	3,66	—	12	—	7.26	—	—
14	9.49	—	—	10,51	13	—	7.45	—	—
15	—	—	—	—	14	11,00	7,04	—	—
16	10,08	—	—	10,35	15	10,18	—	—	—
17	10,06	8,41	2,96	10,00	16	11,03	—	—	—
18	9.54	—	—	10,55	17	—	—	—	—
19	—	—	—	10,12	18	—	—	—	—
20	—	—	—	—	19	—	—	—	—
21	10,39	7,76	1,94	9.93	20	—	—	—	—

TABLE I.

1817.	β Aurigæ Revolutions 35 +	α Cygni Revolutions 24 +	δ Cygni Revolutions 12 +	b Aurigæ Revolutions 12 +
	″	″	″	
May 21	—	—	—	
22	—	—	—	
23	—	6,87	—	
24	—	—	—	
25	—	—	—	
26	—	7,14	—	
27	—	5,84	—	
28	—	—	—	
29	—	—	—	
30	—	6,67	—	
31	—	6,35	—	
June 1	—	—	—	
2	—	—	—	
3	—	—	—	
4	—	5,97	—	
5	—	—	—	
6	—	—	—	
7	—	—	—	
8	—	—	—	
9	—	—	—	
10	—	5,50	—	
11	—	—	—	
12	—	—	—	
13	—	—	—	
14	—	5,78	—	
15	11,03	5,02	—	
16	10,77	—	—	
17		—	0,23	
18	† 10,50	6,32	0,97	
19	12,98	6,73	0,72	
20		6,30	0,96	
21	13,11	6,47	1,46	
22	—	5,84	0,66	
23	—	—	—	
24	—	5,39	0,10	
25	—	4,68	59,40	
26	—	—		
27	—	—	—	
28	—	3,64	58,23	
29	—	—	—	
30	—	3,13	57,72	
July 1	—	—	—	
2	—	3,57	—	
3	—	—	—	
4	—	3,66	57,55	
5	—	3,50	—	
6	—	3,66	58,07	
7	—	3,83	—	
8	—	4,33	58,22	
9	—	4,15	58,38	
10	—	3,26	58,52	
11	—	—	58,32	
12	—	—	57,83	
13	—	3,38	—	
14	—	—	—	
15	—	—	—	
16	—	3,58	59,29	
17	—	—	—	
18	—	—	—	
19	—	3,79	58,00	

1817.	β Aurigæ Revolution 35 +	α Cygni Revolutions 12 +	δ Cygni Revolutions 12 +	b Aurigæ Revolutions 12 +
	″	″	″	
July 21	—	—	58,79	
22	—	3,15	—	
23	—	3,34	58,77	
24	—	2,73	57,97	
25	—	2,98	58,02	
26	—	—	57,76	
27	—	2,97	58,14	
28	—	2,64	57,61	
29	—	—	—	
30	—	3,26	58,08	
31	14,65	2,98	57,78	
Aug. 1	14,34	3,41	58,11	
2	—	—	—	
3	—	3,17	58,70	
4	—	—	—	
5	15,16	3,57	58,42	
6	—	3,97	58,19	
7	—	—	—	
8	15,67	3,27	57,75	
9	—	3,34	57,62	
10	—	2,81	57,91	
11	—	—	—	
12	—	—	57,66	
13	—	—	—	
14	—	3,78	—	
15	—	3,28	57,53	
16	16,34	—	—	
17	14,63	3,18	58,03	
18	—	—	—	
19	—	3,89	—	
20	—	3,31	57,37	
21	:: 14,96	2,36	57,22	
22	14,76	2,83	—	
23	—	—	57,51	
24	—	3,61	58,03	
25	14,25	2,70	57,42	
26	15,12	—	—	
27	14,54	—	57,73	
28	14,64	—	—	
29	15,55	4,11	58,15	
30	14,58	—	—	
31	15,09	3,00	57,91	
Sept. 1	15,46	3,03	57,93	
2	14,24	3,84	—	
3	14,77	—	58,90	
4	14,59	4,00	58,70	
5	14,60	3,55	58,40	
6	—	3,86	58,42	
7	—	3,75	58,76	
8	—	3,47	58,16	
9	—	—	—	
10	—	3,14	58,25	
11	—	3,22	58,28	
12	—	—	—	
13	—	—	—	
14	—	3,13	—	
15	—	—	—	
16	—	—	—	
17	14,58	—	—	
19	15,25	2,53	57,03	
21	15,04	2,81	58,12	

† These observations I consider useless, the star being too near the sun. It was not quite invisible, but was less than the diameter of the wire. This may be remedied another year by the substitution of a cobweb. About this time, likewise, the instrument was much deranged by extreme heat.

TABLE II.

Jan. 21, to March 18.		March 21, to May 16.		July 30, to Sept. 21.	
Winter.		Neutral.		Summer.	
α Cygni	β Aurigæ	α Cygni	β Aurigæ	α Cygni	β Aurigæ
″	″	″	″	″	″
	9,53		10,39	3,26	
8,36	9,65	7,76	9,74	2,98	
8,44	9,84	-8,00	10,25	3,41	
8,18	9,87	8,50	9,73	3,17	
8,25	9,53	8,13	9,72	3,57	
8,19	9,81	8,55	8,95	3,97	14,65
7,76	9,57	8,07	9,27	3,27	14,34
8,15	9,72	8,59	9,14	3,34	15,16
8,24	9,61	8,53	9,70	2,81	15,67
8,74	9,54	8,33	9,57	3,78	14,63
8,51	8,75	7,84	9,48	3,28	14,96
8,33	9,72	8,21	9,52	3,18	14,76
7,80	10,26	7,55	10,30	3,89	14,25
8,36	9,85	8,25	9,95	3,31	15,12
8,28	10,04	8,01	10,10	3,36	14,54
8,31	10,02	8,55	10,45	2,83	14,64
8,00	9,54	8,57	10,38	3,61	15,55
8,53	9,68	7,74	9,89	2,70	14,58
8,00	10,06	7,48	10,71	4,11	15,09
,75	10,16	7,66	8,95	3,00	15,46
7,25	10,16	8,11	10,24	3,03	14,24
7,56	10,27	7,15	10,49	3,84	14,77
8,04	9,91	7,14	11,13	4,00	14,59
8,43	10,46	7,45	9,21	3,55	14,60
8,45	9,49	7,26	10.80	3,86	14,58
8,41	10,08	7,45	11,05	3,75	15,25
	10,06	7,04	11,00	3,47	15,04
	9,54		10,28	3,14	
			11,03	3,22	
				3,13	
				2,53	
				2,81	
Mean Result		Mean Result		Mean Result	
8″.173 + 9″.811 = 59.Rev. + 17″.984.		7″.920 + 10″.044 = 17″.064.		3″.340 + 14″,825 = 18″,165	

TABLE III.

Winter.		Summer.			
α Cygni	δ Cygni	α Cygni	δ Cygni	α Cygni	δ Cygni
″ 8,33	′	″ 6,32	″ 0,97	″ 3,89	″ 58,03
7,70		6,73	0,72	3,31	57,37
8,36	1,51	6,30	0,96	2,36	57,22
8,28	1,85	6,47	1,46	2,83	57,51
8,31	1,15	5,84	0,66	3,61	58,03
8,00	2,83	5,39	0,10	2,70	57,42
8,53	2,55	4,68	59,40	4,11	57,73
8,00	2,38	3,64	58,23	3,00	58,15
7,75	2,67	3,13	57,72	3,03	57,91
7,25	2,88	3,66	67,55	3,84	57,93
7,56	3,22	3,66	58,07	4,00	58,90
8,04	3,82	4,33	58,22	3,55	58,70
8,43	3,66	4,15	58,38	3,86	58,40
8,45	2,96	3,26	58,52	3,75	58,42
8,41	1,94	3,58	59,29	3,47	58,76
7,76	3,39	3,79	58,00	3,14	58,16
8,00	3,65	3,15	58,79	3,22	58,25
8,50	3,54	3,34	58,77	3,13	58,28
8,13	4,53	2,73	57,97	2,53	57,03
8,55	4,07	2,98	58,02	2,81	58,12
8,07	3,24	2,97	57,76	3,97	58,42
8,59		2,64	58,14	3,27	58,19
8,53		3,26	57,61	3,34	57,75
8,33		2,98	58,08	2,81	57,62
		3,41	57,78	3,78	57,91
		3,17	58,11	3,28	57,66
		3,57	58,70	3,18	57,53
Mean Result 8″.165 — 2″ 939 = 12 Rev. + 5″.226.		Mean Result 3″.683—58″.396. = 12 Rev. + 5″.287.			

The winter observations of δ Cygni are very far from satisfactory; the image of the star, from its vicinity to the sun, was smaller than the diameter of the micrometer wire, and it could not therefore be accurately bisected.

TABLES IV. and V.

Table IV. *Showing the effect of parallax on α Cygni.*

21 June	− ″0,78 +	21 Dec.
1 July	0,85	1 Jan.
11	0,88	11
21	0,90	21
1 Aug.	0,88	1 Feb.
11	0,84	11
21	0,77	21
1 Sep.	0,68	1 Mar.
11	0,57	11
21	0,43	21
1 Oct.	0,29	1 April
11	− 0,14 +	11
21	+ 0.01 −	21
1 Nov.	0,17	1 May
11	0,32	11
21	0,46	21
1 Dec.	0,58	1 June
11	0,70	11
21	+ 0,78 −	21

Table V. *Showing the effect of the combined parallax of α Cygni and β Aurigæ.*

21 June	″1,15	21 Dec.
1 July	1,21	1 Jan.
11	1,33	11
21	1,20	21
1 Aug.	1,13	1 Feb.
11	1,05	11
21	0,92	21
1 Sep.	0,77	1 Mar.
11	0,60	11
21	0,40	21
1 Oct.	− 0,20 +	1 April
11	0,00	11
21	+ 0,22 −	21
1 Nov.	0,41	1 May
11	0,60	11
21	0,77	21
1 Dec.	0,92	1 June
11	1,07	11
21	1,15	21

In the above Tables the semi-annual parallax of each star is supposed equal to one second.

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	The Monthly Review for November.	Mr. G. E. Griffith.
	The European Magazine for November.	The Editor.
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	Trattato dello Ariete idraulico del Caval. Brunacci. Milano, 1813. fol.	The Chevalier Brunacci.
	Metereologisches Jahrbuch von 1813, von Canonicus Augustin Stark. Augsburg, 1814. 4°	C. A. Stark.
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	Transactions of the Geological Society. Vol. IV. pt. 1. London, 1816. 4°	The Geological Society.
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Feb. 6.	The European Magazine for January.	The Editor.
	The Monthly Review for January and Appendix to Vol. LXXXI.	Mr. G. E. Griffith.
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	Annals of Philosophy, No. 50.	Dr. Thomson.
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	Annals of Philosophy, No. 51.	Dr. T. Thomson.
	The Philosophical Magazine, No. 226.	Mr. A. Tilloch.
	The Monthly Review for February.	Mr. G. E. Griffith.
	The European Magazine for February.	The Editor.
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	Annals of Philosophy, No. 52.	Dr. Thomas Thomson.
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	An Inquiry into several Questions of Political Economy applicable to the present State of Great Britain. London, 1817. 8°	Mr. Bertolacci, the Author.
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	Annals of Philosophy, No. 53.	Dr. Thomas Thomson.
	The Philosophical Magazine, No. 228.	Mr. Alexander Tilloch.
	The European Magazine for April.	The Editor.
8.	Transactions of the Horticultural Society, Vol. II. pt. 5. London, 1817. 4°	Horticultural Society of London.
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